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PHYSIOLOGICAL EFFECTS OF SCHIZOPHRENIC BODY FLUIDS

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INTRODUCTION

CONTROVERSY has raged for over half a century as to whether schizophrenia, the most severe of all mental illnesses, could have an organic basis or whether its origin lay strictly in the psyche. Over the years, physiologists have probed into almost every aspect of the soma in the quest for some abnormality which would account for the schizophrenic syndrome. Many possibilities have been considered, investigated to a degree and then abandoned when definitive answers were not forthcoming. A dominant approach throughout has been the search for some psychotoxic material circulating in schizophrenic body fluids. Activity has been spurred by the recurrent recognition that chemical agents such as the ergot alkaloids, mescaline, bulbocapnine and lysergic acid diethylamide (LSD-25), could induce in normal persons transient mental disturbances similar to those seen in schizophrenia.

Almost every decade of the present century has witnessed a fresh surge of activity in the search for a schizophrenic psychotoxin, particularly among the aromatic constituents of the body fluids. Around 1916, Holmes (58) suggested that the symptoms might be due to a toxic aromatic amine elaborated by intestinal bacteria. In the 1920s Buscaino (12) supported that view and, by analogy with mescaline and bulbocapnine, believed that the toxin was a phenylethylamine derivative. Hoffer, Osmond and Smythies (56), in 1954, used similar reasoning based on the structures of mescaline, LSD-25, harmine and ibogaine, to propose that the schizophrenic psychotoxin might be an indole derived from adrenaline. Stimulating hypotheses as to the exact nature of the schizophrenic psychotoxin have also been put forth recently by Woolley (121),

Fabing (27) and others. Both old and new hypotheses postulate some aberration in aromatic metabolism, originating either in the body or in the intestinal tract and resulting in the production of a psychotoxic material.

Attempts to demonstrate the validity of this psychotoxin hypothesis have traditionally followed two major lines. The first is the attempt to show some abnormality in the aromatic content of schizophrenic body fluids by application of chemical techniques. These have ranged from crude tests, such as that for indican used by Townsend (106) or Buscaino's "black silver nitrate reaction" (13), to the more refined chromatographic techniques applied to the problem today. The end result of the chemical work to date has been the demonstration of a probable quantitative and a possible qualitative dysfunction in aromatic metabolism in schizophrenics. The nature and importance of this dysfunction are unknown and its very reality is still seriously questioned by many. The second approach has been the attempt to demonstrate abnormal physiological (i.e. psychotoxic) effects of schizophrenic body fluids. This paper is intended to review some of the many reports in the literature dealing with this second approach to the problem. The reviewers have not attempted to include all the material on the subject, nor have they restricted the scope only to reports containing substantial experimental evidence to back the conclusions. The chief criterion in selection has been the probable interest to researchers currently working in the field.

The difficulties inherent in devising a meaningful test for "abnormal" effects of schizophrenic body fluids cannot be over-emphasized. The symptoms of schizophrenia and the criteria for diagnosis are almost exclusively psychological, whereas most animal tests are essentially physiological. This combination lends itself poorly to definitive investigation. Psychiatrists still debate as to what constitutes the clinical entity schizophrenia; it is not surprising, therefore, that the subjects of experimental psychosis and psychotoxicity are extremely controversial, and that the requisites for an appropriate test of these phenomena become almost a matter of personal conviction. Over the years many different kinds of tests have been used and, for convenience in discussion, they have been classified as:

- (A) Elicitation of "catatonic" symptoms in animals.
- (B) Occurrence of other "C.N.S." effects in animals such as a change in a trained response or in behaviour.
- (C) Toxic effects towards animals, plants, cell cultures and other organisms.
- (D) Hyperglycaemic effect in animals.
- (E) Toxic effects on enzyme systems *in vitro*.

The relationship to the schizophrenic problem in some of these groups seems remote. The assumption is usually made, however, that the test is of significance if differences between schizophrenic and normal body fluids can be demonstrated. Based on this assumption, there have been many reports of "successful" tests using blood, bile, C.S.F. and urine. The results have been variable, and there has been much question as to the reality of the effects observed (cf. Table I). Part of this inconsistency may be attributed to the necessity for subjective evaluation in many of the tests and part to variation in biological sensitivity of the test organisms. Much of the inconsistency, however, results from shortcomings in experimental design. Certain of these weaknesses should be emphasized at the outset because they apply so generally to the tests reported to date.

TABLE I

Physiological Effects of Schizophrenic (as Compared with Normal) Body Fluids

Test		Workers Reporting "Abnormalities" for Schizophrenic Fluids	
Body Fluid		Positive Findings	Negative Findings
I. Catatonic Symptoms in:			
Mice	Urine	Nieuwenhuyzen (1934)	Tinel and Eck (1933)
Mice, pigeons, etc. ..	Urine	Baruk and Camus (1933-49)	Tomesco (1937)
Monkeys and cats ..	Urine extracts	Wada and Gibson (1957)	
Mice, pigeons or cats ..	Blood	Baruk and Camus (1933-49)	Guilmot (1952)
Mice, pigeons, etc. ..	Bile	Baruk and Camus (1933-49)	
Mice	Bile	DeNigris and Mariani (1936)	
Mice and cats	Bile	Guilmot (1952)	
Rats	C.S.F.	LeGrand and Annee (1938-39)	Shapiro (1956)
Guinea pigs	C.S.F.		Sogliani (1938)
II. Schizophrenic Symptoms in:			
Monkeys and man ..	Serum fraction	Heath <i>et al.</i> (1956)	Smith (1958)
III. Behavioral Changes in:			
Siamese fighting fish ..	Urine		Smith <i>et al.</i> (1956)
Spiders	Urine extract	Rieder <i>et al.</i> (1957)	
Rats	Serum	Winter and Flataker (1957)	
IV. Lethality to:			
Rabbits	Urine	Bouchard (1887)	
Rabbits	Urine	Juschtschenko (1909)	
Guinea pigs	Urine	Loewe (1911)	
Mice	Urine and C.S.F.	Gamper and Kral (1934)	
Rabbits	Urine extracts	Moya <i>et al.</i> (1956-58)	
Mice	Urine extracts	Kemali <i>et al.</i> (1957)	
Mice	Serum	Weichbrodt (1922)	
Mice	Serum	Kastan (1926)	
Mice	Serum	Baldi (1926)	
Mice	Serum	Wahlstrom (1936-37)	
Mice	Serum	Miyakawa and Yamasita (1937)	
Mice	Serum	Wilczkowski (1946)	
Mice	Serum	Sjovall (1947)	
Mice	Serum fraction	Mall (1958)	
V. Toxicity to:			
Tadpoles	Serum and urine	Lazell and Prince (1929)	
Tadpoles	Serum and urine	Malis (1947)	Edisen (1956)
Tadpoles	Serum and urine	Fischer (1952-56)	Georgi <i>et al.</i> (1954)
Yeast cells	Serum, urine, C.S.F.	Rieder (1953-57)	
Paramecium	Urine extracts	Weber (1953)	
Cell cultures	Serum	Federoff (1956-58)	Reiter (1938)
Plants	Serum, urine, C.S.F., sweat	Macht (1924-56)	
Plants	Urine	Herz and Weichbrodt (1924)	
Plants	Urine	Tscherkes and Mangubi (1931)	Freeman and Looney (1934-39)
Plants	Urine	Hoffer (1955)	
VI. Inflammatory Action in:			
Rabbit's eye	C.S.F.	Gamper <i>et al.</i> (1932-34)	Scheid (1937)
Rabbit's eye	C.S.F.	Dussik and Pichler (1938)	Reiter (1938)
Rabbit's eye	C.S.F.		Persic and Feric (1957)
VII. Hyperglycaemic Effect in:			
Rabbits	Serum	Hall (1938)	Goldner and Ricketts (1942)
Rabbits	Serum	Meduna <i>et al.</i> (1942)	Harris (1942)
Rabbits	Urine extracts	Meduna and Vaichalis (1948)	
Rabbits	Urine extracts	Morgan and Pilgrim (1952)	
Rabbits	Urine extracts	Shiraishi (1955)	
Rabbits	Urine extracts	Moya <i>et al.</i> (1955-58)	
VIII. Inhibition of Glucose Utilization by:			
Rat diaphragm	Serum	Walaas <i>et al.</i> (1954)	
Rat neural tissue	Serum	Hukuda (1936)	
Rat neural tissue	Serum	Haavaldsen <i>et al.</i> (1957)	
Rat neural tissue	Serum	Streifler and Kornbluth (1957)	

1. The number of tests run is often too small to eliminate entirely the factor of chance.
2. Schizophrenia is usually presumed to be a single disease entity which runs a constant course. Adequate description of the criteria for original diagnosis, severity and acuteness of the symptoms, stage of the disease and physiological status of the individuals comprising the schizophrenic group are seldom given.
3. Diet, stress and similar factors are frequently not comparable between the schizophrenic and the control groups, and what part such factors may play in the test results is unknown. Horwitt (59) believes that inattention to such information is the greatest weakness in all physiological research in schizophrenia.

4. Specificity of the test to schizophrenia is seldom shown. Comparisons are usually made between schizophrenics and either non-schizophrenic mental patients or normals. Other common disease processes, with no associated mental symptoms, are usually not shown to give negative results. This objection seems particularly important when the significance of most tests to schizophrenia must be assumed.
5. The possibility that the test results may be due to differences in pH, tonicity, electrolyte distribution, buffering action or similar "artefacts" is not always considered. One might assume that the schizophrenic and control fluids would average the same in these respects but this assumption may be erroneous, particularly in work with urine.

REVIEW OF EXPERIMENTAL WORK

A. *Production of Experimental Catatonia in Animals and Humans*

Catatonia is probably the only biological effect which can be readily associated with schizophrenic symptomatology. The association may, perhaps, be too easily made, since the catatonic effect is notoriously non-specific. De Jong (16), in an excellent review of this phenomenon, describes it as a general reaction form of the central nervous system. Catatonia, with its most significant component, catalepsy, can be induced in many animal species by drugs such as bulbo-capnine, cannabis, mescaline and substances related to adrenaline, by asphyxiation, by gases such as carbon dioxide and by neurosurgical, physical and other means.

Non-specific as the effect may be, catatonia has been reported as being produced in animals and in man through the use of schizophrenic but not normal body fluids, and this stands out as the most dramatic evidence so far advanced that schizophrenia may involve the production of psychotoxins.

De Jong and his group (18, 21) did some of the earliest work on the production of catatonia in animals, using human body fluids. They found that a benzene extract of urine contained a substance, tentatively called "catatonine", which was much more potent than mescaline, bulbo-capnine, adrenaline or acetylcholine in evoking catatonic symptoms in rats and mice; it was ineffective in rabbits. "Catatonine" was found to exist in less than normal amounts in the urine of mental patients, and from this it was originally argued that "catatonine" was retained by schizophrenics. Injection into rats of urine extracts containing "catatonine" led to catalepsy of the rear paws. Similarly active extracts could also be obtained from blood (22). A quantitative bio-assay was worked out and "catatonine" was isolated from urine. It proved to be chemically identical with nicotine (37), and in retrospect it was established that the mental patients tested smoked less than the normals. This is a good illustration of one of the many pitfalls plaguing investigators in this field.

Later Nieuwenhuyzen in de Jong's group found that, if the urine of non-smokers was used, there remained a catatonogenic agent which appeared in greater amounts in the urine of schizophrenics than of non-schizophrenics. This agent was not identified. It could be demonstrated in benzene extracts of mildly alkalinized urine in 14 out of 24 (58 per cent.) of schizophrenics; the method of obtaining and testing the extract was critical (17, 83). Nieuwenhuyzen also showed that the "oxypoteic acid fraction" of schizophrenic urine, which is obtained by precipitation as a barium salt, led to catalepsy and hyperkinesis when injected into mice. About 6 mg. of this preparation was required as compared to about 2 mg. of histamine, 6 mg. of tryptamine, 0.04 mg. of nicotine and

150 mg. of urea (84). This oxyproteic acid fraction of urine was examined by Gullotta (49) in his studies on aromaturia in schizophrenics.

Tinel and Eck (104) reported a catatonogenic, thermolabile and easily oxidized lipoid in human urine which occurred to about an eight-fold greater extent in normal than in catatonic urine. They indicated that this material was not identical with "catatonine" but details are lacking and the report is unconfirmed.

More recently, Kemali (65) has found occasional evidence of catatonic symptoms when a "chromogen" fraction of schizophrenic urine is injected into mice; death, however, is the more frequent result (see Section C).

Baruk, one of de Jong's early collaborators, went on to explore the catatonogenic properties of schizophrenic body fluids in a whole series of investigations (5, 6, 14). Baruk and Camus reported that they could induce catatonia with typical posturing by subcutaneous injections of urine, blood or bile from schizophrenics; they found that bile gave the most consistent results (cf. 102). The effects were variable and depended on the animal species used. Clear catatonia lasting episodically for several hours, or even days after injection, was produced in mice given $\frac{1}{2}$ –1 c.c. of bile. Cats given 8–10 c.c. showed variable effects and guinea pigs (4–6 c.c.) showed little reaction. Some birds, such as pigeons (3–4 c.c.), also showed catatonia; other species showed only torpor. During non-catatonic intervals there was frequently ferocity, excessive salivation, muscular twitching and hyperreactivity of tendon reflexes. However, bile from some cases of serious migraine, icterus catarrh, chronic rheumatism or "hepatic-intestinal intoxication" sometimes also proved catatonogenic. Normal human or animal bile never produced anything more than torpor and somnolence. Tests showed that these minimal effects could be attributed to known bile constituents (bile salts, sodium taurocholate, sodium glycocholate, cholesterol, amino acids, gastric sugar, etc.); none of these normal bile constituents tested in pure form produced anything more than torpor and sometimes dyspnoea. Cholesterol seemed to inhibit the catatonic effect of active biles. Biles from anxiety patients produced dyspnoea but no catatonia. The active agent seemed to be thermolabile and benzene-soluble. Baruk and Camus believed that the toxin was produced by intestinal coliform bacteria and, following this lead, DeNigris and Mariani (19) reported that filtrates of the cultures of such bacteria obtained from the faeces and bile of catatonics produced acute and chronic catalepsy with periods of exaltation in animals.

Tomesco and his co-workers (105) also followed Baruk in demonstrating catatonia, as evidenced by maintenance of unstable equilibrium in white mice, after intra-peritoneal or subcutaneous injection of filtered, colibacillus-containing urine from a catatonic schizophrenic. The effect lasted 1–3 hours depending on the dose used ($\frac{1}{2}$ –3 c.c.) and was consistent in the numerous mice tried. C.S.F. from the same schizophrenic was not active and no sign of catatonia was evoked in any of the 15 animals injected with physiological saline, 2 normal urines or 2 schizophrenic urines in which colibacilli could not be demonstrated. The toxic urine retained its activity for 3–4 days at room temperature; heating the urine to 120° C. for 30 minutes resulted in an increase in toxicity but a modification of the catatonic action. The intestinal intoxication hypothesis, held by these various workers, would seem untenable today. Intestinal sterilization of schizophrenics, which has become possible since the time of Baruk and Camus, has not produced notable clinical benefits.

More recently, Guilmo (48) injected bile subcutaneously into the abdominal region of cats (10–20 c.c. dose) or mice ($\frac{1}{2}$ c.c.). He used persistent

placement of paws, a lack of response to stimulation and a conservation of attitude in unstable equilibrium as the three signs of catalepsy. He treated 17 animals with bile from catatonic schizophrenics, 3 with normal bile and 4 with bile from miscellaneous non-schizophrenic mental patients. Somnolence was a common symptom with both schizophrenic and non-schizophrenic biles. None of the non-schizophrenic biles caused death and only one of the 7 evoked any sign of catalepsy. By contrast, 9 of the 17 animals receiving catatonic bile showed all three signs of catalepsy, another 4 of these 17 showed one or two signs of catalepsy and 3 of the 17 died. Although the group is small, the distinction between bile from catatonics and from normals seems clear. Blood from a catatonic produced no effect, and none was obtained with 50 mg. of progesterone or with 0.5 mg. of nicotine. This last is surprising in view of de Jong's findings. The effects with bile came on $\frac{1}{2}$ to $1\frac{1}{2}$ hours after injection and sometimes lasted 12 hours or more. There was a great variation from animal to animal.

LeGrand and Année (67) found that catatonic symptoms could be produced in rats by subcutaneous injection of lyophilized material from schizophrenic C.S.F.; the symptoms lasted 48 hours and were elicited by samples from 10 out of 13 schizophrenics and by 0 out of 6 normals and epileptics. The activity was destroyed by heating to 45° C. (but not to 40° C.) for 30 minutes. There was no loss of activity on dialysis, filtration or ultracentrifugation. Shapiro (96) attempted to repeat this work using C.S.F. from chronic schizophrenics of 6 to 25 years duration but found no such consistent effects; he found the main reaction was listlessness due to hypertonicity of the injected solutions and was elicited by C.S.F. from mental defectives as consistently as with C.S.F. from schizophrenics. The difficulty may have been in his choice of chronic cases since almost all investigators have found that the body fluids of acute cases show "abnormal" physiological effects more strongly than do the body fluids of chronic schizophrenics. Sogliani (101) also reported minimal reactions in guinea pigs or rabbits injected with schizophrenic C.S.F.; these animal species proved insensitive in the hands of de Jong and of Baruk. In contrast to Guilmet, Sogliani found that schizophrenic blood caused symptoms identical with those produced by bulbocapnine but that the same reaction could often be obtained with the blood of normals.

Interest in catatonogenic effects has recently been revived by the dramatic reports of Heath and his co-workers (53, 68). They found that catatonic and other "schizophrenic" symptoms could be produced in monkeys and man by injection of a material from schizophrenic serum. The material, named taraxein, is an enzyme related to but not identical with the copper-containing enzyme ceruloplasmin. Both enzymes oxidize adrenaline; *in vitro* tests show that schizophrenic serum oxidizes adrenaline (added in approximately 1,000,000 times physiological concentrations) more rapidly than does normal serum (52). According to Hoffer and Kenyon (55), the oxidation product in this *in vitro* system is adrenolutin or some related hallucinogenic material. Attempts by Leach and Heath to isolate the oxidase enzymes in serum led at one stage to a blue precipitate being found in schizophrenic but not in normal serum. It is this fraction that yields the taraxein which, on injection, leads to profound EEG and behavioural changes in monkeys and to "schizophrenic" symptoms in man. About 400 ml. of serum must be processed to obtain the material for one test. The material is unstable, being inactivated by a number of factors.

The number of tests reported so far is small and the results somewhat variable. Material which was screened for activity in monkeys has been given to

17 non-psychotics and to 3 psychotics in remission; the experiments were double blind with a number of inert substances serving as controls, including enzyme preparations which proved inactive in the monkey screen. The effects of taraxein on psychotics were more pronounced than on normals. In normals the peak effect occurred 15-40 minutes after injection and lasted 4 hours at most; in psychotics the effects lasted in some cases for four days. Although taraxein appears to be absent in normal serum, its presence or absence has not been reported for other disease states in which rapid *in vitro* oxidation of adrenaline by serum has been shown.

One argument raised against Heath's work is that transfusions of schizophrenic blood do not elicit the reactions reported for taraxein*. It can be argued, however, that the difference resides in the speed of administration of the toxic material. It is known, for example, that the effects of bufotenine depend on the speed of injection as well as on the dose given (28). Heath has given rapid transfusions of schizophrenic serum to 4 non-psychotics and reported the production of transitory mental disturbances. As Bond (9) points out, this is a simple experiment which should open for other laboratories a way to confirm Heath's work. Although the response of normals to this procedure may be transitory, the effects on psychotics in remission should be more intense and thus readily observed. Early efforts to confirm the work in humans have been disappointing (99, 122), although Melander *et al.* (125) have recently reported reproducing Heath's findings "on several occasions". As yet, detailed data are not available from these other laboratories working with taraxein. Judgment as to the significance of this work must wait on a great many more experiments. The special problems associated with defining schizophrenia and its symptoms, as well as the relationship of such factors as stage of the disease, stress and nutritional status to the effects observed, make an early answer unlikely. Certainly the promise of the work justifies exploration by many laboratories.

Wada and Gibson (111) have studied behavioural and EEG changes in cats and monkeys injected intracisternally or intraventricularly with extracts prepared by McGeer *et al.* (71) of the aromatic constituents of normal and of schizophrenic urine. Both types of extract caused some degree of docility, somnolence or torpor in animals. The schizophrenic extract brought about various unusual behavioural patterns, ranging from rage states (5/10 cats), and automatism-like states (3/10 cats) to recurrent stuporous and catalepsy-like episodes (1/10 cats, 5/10 monkeys). Except for one brief stuporous episode in one monkey, none of the 9 cats or 11 monkeys showed such unusual behaviour following injection of a normal extract even at higher dosage level. The amount of schizophrenic extract used corresponded to 1-5 ml. of urine. Most of the experimental animals were used for both normal and schizophrenic extract in order to minimize the effect of differences in sensitivity. EEG changes were also much more pronounced after injection of schizophrenic extract.

B. Occurrence of Other Essentially C.N.S. Effects in Animals

1. *Behavioural Changes.* Winter and Flataker (117) developed a rope-climbing test for rats in order to compare responses to various antihistaminic drugs. Trained rats will climb a vertical rope in a given time with only minor variations from trial to trial. The "climbing time" can be measured to 0.1 second. Injection of the hallucinogen, LSD-25, into these rats leads to a delay

* Two groups report therapeutic benefits in schizophrenics treated by blood transfusions (8 of 12 in one series and 5 of 6 in the other (44, 123)), but another group found such treatment useless in their series of 5 cases (86) (cf. 128).

in climbing time (118), as does intra-peritoneal injection of 1 ml. of plasma. The average delay caused by plasma from 46 normals or from 35 non-psychotic medical and surgical cases was very slight and only about one-sixth that caused by plasma from 80 psychotics, the majority of whom were schizophrenics. The difference between the normal and schizophrenic groups is statistically highly significant. The rats receiving active schizophrenic plasma also showed behavioural changes; they retreated to the back of the cage, huddled together and became quiet and withdrawn. Their climbing technique was clumsy and the animals often stopped and hung "bewildered" on the rope. Subcutaneous injection of plasma or intra-peritoneal injection of schizophrenic C.S.F. produced no such effects. Ceruloplasmin, but not albumin, potentiated the effect of serum in rats. Adrenergic blockade modified the serum effect markedly but cholinergic blockade did not (119).

Another attempt to produce an LSD-like effect in animals has been made by Rieder (92). Following a report of Witt (120) that administration of LSD-25 and other hallucinogens to spiders led to characteristic changes in the type of web, Rieder induced similar, but not identical, changes by giving extracts of the basic components of both normal and schizophrenic urine. The active material(s) in normal urine could be extracted into chloroform while that in schizophrenic urine appeared principally in a butanol extract of the aqueous residue from the chloroform extraction. Rieder and others (126, 127) feel that this spider web "test" does not reflect the effect of a single "psychotoxin" but probably the combined action of a number of substances.

Smith and her co-workers (100) reported that the characteristic response of Siamese fighting fish to LSD-25 (26, 108) was not produced when the fish were exposed to 25-50 per cent. solutions of urine from either schizophrenics or normals.

Bulle and Konchegul (11) found that the C.S.F. of all 17 schizophrenics tested resembled 5-hydroxytryptamine (serotonin) in altering the pain response and knee jerk of dogs on injection into the internal carotid artery; C.S.F. samples from manic depressive or neurotic patients had some, but dissimilar, effects, and C.S.F. samples from normal persons had no effect.

EEG changes in normal humans are reportedly induced by intravenous injection of 10-20 ml. of epileptic or schizophrenic C.S.F. (89). The reality and meaning of this effect must await further details and confirmation.

2. *Modification of Adrenaline Effect.* Local application of adrenaline to the parietal lobe of a dog leads to an increase in blood pressure which apparently depends on liberation of a hypertensive agent by action of the hypothalamus; this reaction is strongly modified by chlorpromazine or reserpine. Dogs injected with schizophrenic serum do not show this response to parietally applied adrenaline (though i.v. adrenaline still causes a rise in blood pressure). Dogs injected with normal serum do show response to parietally applied adrenaline. The refractoriness of dogs after schizophrenic serum is believed due to a polypeptide substance P. The brains of normal dogs contain about 23 units of substance P per gram of tissue; the brains of dogs injected with schizophrenic serum contain 43-116 units per gram with local concentrations of 275-312 units/gram in the hypothalamus (76).

C. *Toxicity to Animals, Plants, Cell Cultures and Similar Organisms*

1. *Work with Mice, Rabbits and Similar Animals.* A number of reports are in the literature indicating that schizophrenic body fluids are more toxic than

normal towards various animals. Unfortunately the information available from some of the early work does not make clear what criteria of toxicity were used or how many experiments were performed with fluids from what type of patient.

(i) Work with urine. In 1909 Juschtschenko (63) reported that the urine of psychotics was more toxic than that of normals and correlated this with a "decreased somatic oxidizing ability" as measured by a decreased excretion of phenol after a test dose of benzene. Previously Bouchard (10) had reported that both the urine and blood of psychotic patients was more toxic than that of normals to rabbits. In 1911 Loewe (69) found that the urine of schizophrenics, and particularly of catatonics, was more toxic towards guinea-pigs than was normal urine. Some toxicity was noted in the urine of demented alcoholics, persons with progressive paralysis and epileptics. The "toxic factor" in urine was non-dialysable.

Abely *et al.* (1) reported that injection of psychotic urine into guinea-pigs resulted in weight loss and histological changes in the seminal vesicles but that psychotic C.S.F. had no consistent effect (cf. 15, 129, 130).

In 1934 Gamper and Kral (39) found that many samples of both normal and schizophrenic urine were toxic to mice on subcutaneous injection. In contrast to Abely, however, their major effort was on the toxic effects of C.S.F. (Section C-1 (iii)). Very recently, Moya has noted, as an incidental finding, a high percentage of deaths in rabbits injected with schizophrenic urine extracts (see Section D).

Kemali (65) has isolated a "chromogen fraction" from overnight urine by adsorption on to carbon and elution at 100° C. with an ammoniacal organic medium. Portions of this extract corresponding to 10-30 c.c. of urine were injected intra-peritoneally into mice. Schizophrenic extracts invariably caused restlessness, ataxia, hypotonia and death; normal extracts, even at double or triple the dose, caused only temporary restlessness and a short prostration with complete recovery in about one hour. Fractionation of the crude schizophrenic extracts by column chromatography has yielded several very highly toxic fractions.

Pfeiffer and Albrecht (87) reported that schizophrenic urine was much more "toxic" than normal urine; they used the temperature decrease produced in experimental animals multiplied by the time of temperature deficit as a measure of "toxicity". Urechia and his co-workers (110) were not only unable to confirm this report but they found a completely opposite effect. According to them, all urines examined contained a pyretogenic substance, believed to be a polypeptide with sugar and chondroitin sulphate components, but the excretion of this substance was markedly increased in schizophrenia as well as in pregnancy and in kidney disease.

(ii) Work with serum. In 1922 Weichbrodt (115) found that the blood and serum of many patients suffering with endogenous psychoses caused death in immature white mice when injected intra-peritoneally but not when injected subcutaneously. The toxic factor was destroyed by heating at 56° C. for 30 minutes. The blood of menstruating women and of epileptics before and during attacks was also toxic but no toxicity was found in blood from paretics, arteriosclerotics, senile psychotics or hysterics.

Kastan (64) and Wilczowski (116) both confirmed Weichbrodt's results although the former found toxic sera in senile psychotics as well as in schizophrenics and epileptics. The mice injected with toxic sera showed increased neutrophils according to Kastan. Wilczowski found that serum was more

toxic than whole blood and that the toxicity was destroyed by heating to 35° C.

Wahlstrom (112) and Sjoval (98) also confirmed Weichbrodt except that they used different routes of injection. Wahlstrom found that only 10–12 per cent. of mice injected subcutaneously with sera from 60 normals died as compared to 34 per cent. injected with sera from 561 schizophrenics. As reported by Weichbrodt, the toxicity was destroyed by heat. Ether-extracted serum was not toxic, nor was the extract itself, but a combination of the ether extract plus a non-toxic serum was toxic. This indicates that two serum components are needed for toxicity. A similar indication was found by Federoff (29) in work with cell cultures (see Section C-4). Mice treated with toxic sera showed pathological changes in the liver and brain. Histological changes have also been noted in the livers of guinea-pigs injected with schizophrenic C.S.F. (20) and in the brains of dogs injected with blood from "acute psychotics" (8).

Sjoval (98) felt that Wahlstrom's conclusions were questionable on statistical grounds, particularly because Wahlstrom had used a varying number of mice per serum. Sjoval therefore repeated the work using sera from 24 unmedicated, male, hebephrenic and catatonic schizophrenics and 24 male hospital attendants. Physical and laboratory examinations were made to ensure that all were somatically healthy. Each serum was tested in 30 mice at an i.v. dosage of 0.025 ml./gram. Mice injected with lethal sera died within 24 hours. The mean number of deaths per sera was 23 out of 30 for the schizophrenic sera and only 15 for the normal sera. The standard dosage was below the LD₅₀ for only 6 of the schizophrenic sera, including four from inactive cases, whereas 12 of the 30 normal sera were below the LD₅₀. Rapidity of death, which seemed to be connected with a haemolytic principle, was also greater with the schizophrenic sera. Sjoval concludes that the difference in toxicity between schizophrenic and normal sera is statistically significant.

In independent work, Miyakawa and Yamasita (77) also found that schizophrenic sera on intra-peritoneal injection were lethal to mice in a high percentage of cases while sera from normals or non-schizophrenic psychotics were not. Sera from acute schizophrenics were much more toxic than those from chronic cases but there was no significant difference between sera from hebephrenics and catatonics. This latter detail conflicts with the report of Baldi (4) who found that while 3–5 c.c. of catatonic serum per kilogram were needed to evoke a toxic reaction on intravenous injection in rabbits, sera from hebephrenics (6–7 c.c./kg.) and paranoid schizophrenics (8–10 c.c./kg.) were progressively less toxic. Similar toxicity in a mild form was shown by sera from some medical cases including patients with haematuria and liver disease.

Mall (124) found that trypsin-digested sera of some acute or catatonic schizophrenics, as well as epileptics, cause convulsions and death on injection into mice. Untreated sera or trypsin-digested normal sera had no such activity. The toxic material seems to be associated with the α -2-globulin fraction and there is some indication that normal sera may also contain the toxic component which is inactivated by a protective component of the albumin fraction.

Injection of schizophrenic sera into salamanders leads to an increase in capillary permeability which is indicated by a disappearance of fluorescein from the blood stream. Normal sera had no effect but similar changes were found with sera from cases of acute nephritis or severe infections. The effect was compared to that of histamine (25), which is interesting in that a significant hyper-histaminaemia has been reported for schizophrenics (46).

(iii) Work with C.S.F. Gamper and his co-workers found that C.S.F. from

many schizophrenics caused death on subcutaneous injection into immature mice and inflammation when injected into the anterior chamber of a rabbit's eye. There was a clear distinction between the toxicity of schizophrenic C.S.F. and that from normals, manics or other types of non-organic psychoses. Toxic C.S.F. was obtained from epileptics, alcoholics, senile psychotics, cases with various organic brain disorders and some carcinoma patients. Two per cent. saline was non-toxic. The C.S.F. samples from hebephrenic and catatonic schizophrenics were generally much more toxic than those from paranoid schizophrenics. Several hundred samples were apparently studied in a number of experimental series (39, 40, 41).

In a smaller series of 18 schizophrenics, most of whom were paranoid, Dussik and Pichler (23) found that the C.S.F. caused less inflammation than reported by Gamper and Kral but that there was a clear reaction elicited by 61 per cent. of the samples. In 9 of 10 cases studied serially, the direction of change of the inflammation reaction paralleled the clinical change under insulin treatment.

Unfortunately, neither Scheid (95), nor Reiter (90) could confirm Gamper and Kral on the toxicity of schizophrenic C.S.F. to animals. Persic and Feric (85) tested C.S.F. samples from 42 schizophrenics and from 20 patients with other nervous disorders in the anterior chamber of rabbit eyes. They found a severe inflammatory reaction with 34 per cent. of the samples but this included a higher percentage of the non-schizophrenic than of the schizophrenic samples.

2. *Tests with Tadpoles.* Fischer (33, 35) reported a significant difference in toxicity to tadpoles between the serum and urine from acute schizophrenics and that from normals. The toxicity decreased as the schizophrenics improved under treatment. Lazell and Prince (66) found a similar difference in toxicity between normal and schizophrenic serum and Malis (73) is said to have obtained similar results; Edisen (24) reported inconclusive results using a tadpole test. Fischer (35) later said that the toxicity was related to the ammonia content of urine and that the test must be performed with urine of a standard salt concentration and at a standard barometric pressure. He felt that the test results correlated with the degree of stress.

The validity of this tadpole test has been the subject of a somewhat acrimonious polemic between Fischer and his erstwhile colleagues in Georgi's laboratory in Switzerland (42, 34). The latter group point out that the great variability in the data of Fischer collected in Georgi's laboratory, is not related particularly to weather conditions but probably depends on the reaction of tadpoles to various non-specific environmental factors (cf. 47). In their evaluation of Fischer's data on 34 schizophrenics and 34 normals, the schizophrenic urine and sera were not significantly more toxic than the normal (91).

3. *Work with Micro-organisms.* After discarding the tadpole test as of doubtful value for the demonstration of a specific toxic factor in schizophrenic body fluids, Rieder (91) tried yeast cells (*Saccharomyces cerevisiae*) as a test organism. He measured in a Warburg apparatus the oxygen uptake of the cells on exposure to dilute serum, urine or C.S.F. Care was taken to evaluate and minimize the effects of pH, solution tonicity and similar environmental "artefacts". His initial report was based on results with samples from 68 schizophrenics, 49 normals and 64 non-schizophrenic mental patients. Variability within the normal group was within acceptable limits and repeat tests on a single individual, whether schizophrenic or non-schizophrenic, gave closely similar results. On the average, the schizophrenic samples of urine,

C.S.F. or serum showed highly significant inhibition of yeast metabolism as compared to normals. Of the 64 samples from the non-schizophrenic mental group, only 8 per cent. showed significant inhibition and most seemed stimulatory. The non-schizophrenic samples came from 14 epileptics, 25 multiple sclerotics, 7 chronic alcoholics and 18 post-encephalic Parkinsonism patients. When more than one body fluid from a single patient was tested, there was good agreement between the values obtained for the different liquors. Overall, however, schizophrenic urine seemed slightly more inhibitory than either serum or C.S.F.

Weber (114) studied the toxicity towards *Paramecium* of basic fractions obtained from urine by absorption and elution from IRC-50 columns followed by electrophoresis. The fractions from schizophrenic urines were more toxic than those from normal urines with a highly significant difference in the fractions of ionic mobility comparable to that of mono- or dimethylamine. Fischer (34) implied that the toxicity factor tested for by Weber was ammonia, but ammonia, mono- and dimethylamine were excluded by testing the toxicity of solutions of the pure substances.

4. *Work with Cell Cultures.* Although Reiter (90) twenty years ago could not detect any significant differences between osteoblasts grown in schizophrenic and in normal sera, Federoff and his colleagues (30, 31) have recently shown an overall difference in toxicity to certain cell strains between schizophrenic and non-schizophrenic sera. The sensitivity of cell cultures towards schizophrenic sera varies greatly with the cell strain. Fifty-nine schizophrenic sera out of 80 showed very high toxicity to L strain cells compared with only 3 out of 29 normal samples. In another series the sera from 33 healthy adults, 32 non-schizophrenic mental patients, 98 schizophrenics and 28 surgical patients were examined. The normal sera were clearly less toxic than sera from any of the other groups. There was a significant difference between the schizophrenic and non-schizophrenic mental patients which seemed even more marked when the frequency of occurrence of the Lewis and Piotrowski "signs of schizophrenia" was compared with the toxicity score. The surgical patients as a group showed a toxicity comparable to that found with the schizophrenics but 10 of the 17 surgical patients whose sera were very toxic had urinary retention (32).

Although toxic sera could be stored at -20°C . or in the lyophilized state, toxicity was lost if the serum was heated to 56°C . for 35 minutes. The activity was not destroyed by dialysis or by mechanical shaking. Toxic serum which was deactivated by heating could be reactivated by addition of a non-toxic human (but not animal) serum. This indicates that at least one heat-labile and one heat-stable component are necessary for toxicity and that the former is also found in normal, atoxic human serum (29). The multiple nature of the toxic factor in sera, implied by this work and by that of Wahlstrom (see Section C-1), complicates considerably the problem of isolation and identification.

Federoff reported that the morphological changes resulting from the action of toxic sera on fibroblast-like cells in tissue culture are strikingly similar to the cytotoxic reaction produced by anti-serums but there is no conclusive evidence that the toxic substances in human serum are immunologically active (29, cf. 98, 109).

Georgi and Rieder (43, 93) have tested the effects on tissue cultures of chloroform and butanol extracts of alkalinized urine and of the desalted aqueous residue. The growth-inhibiting actions of the schizophrenic fractions were not significantly abnormal, although the schizophrenic chloroform

fraction may be slightly more toxic than the normal when compared on an equal "free base content". The butanol fractions of multiple sclerotic urine show a greater growth-inhibitory effect than do the corresponding fractions of either normal or schizophrenic urine. Multiple sclerotic urine was previously shown to have a stimulating effect on yeast cell metabolism as contrasted with the inhibitory effect of schizophrenic urine (91).

5. *Work with Plants.* Sera, red cells, C.S.F., urine, sweat and saliva from all types of psychotic patients have been reported to inhibit the growth of pea seedlings (*Lupinus alba*). A similar, but not identical, growth inhibitor is reported in the body fluids of patients with cancer, pernicious anaemia and certain other diseases, as well as in the body fluids of menstruating females. No such growth-inhibiting effect is found with fluids from normal, healthy persons or the general run of medical cases. Tests with indican and with various phenols known to occur in normal urine indicate that they have no effect. The toxicity of sera or other fluids from mental patients is destroyed by boiling or by X-irradiation but this is not true of the toxicity of fluids from cancer patients, menstruating females, etc. Whole blood is more toxic than sera or red cells alone, which is in contrast to the findings of Wilczowski (116) who reported serum much more toxic than whole blood to white mice. Over 1,200 samples have been tested by Macht to reach these conclusions (72). Similar effects on root growth have been reported by Hoffer (57), by Tscherkes and Mangubi (107) and by Herz and Weichbrodt (54). This last group found a parallelism between the phytotoxicity of schizophrenic sera and the toxicity on intraperitoneal injection into mice. However, some other workers (36, 70) could not confirm the phytotoxicity of psychotic blood and urine towards *Lupinus alba*.

Jacobowsky (62) tested the phytoactivity of blood sera on a mould (*Aspergillus niger*). He studied sera from 59 individuals in one series and from 53 in a second series. All were male and somatically healthy. The 44 "normal" hospital attendants were eating the same diet as the 61 schizophrenics and 11 non-schizophrenic mental patients. Jacobowsky found that all sera stimulated growth; no toxic effect was detected. On an average, within each series, the sera from non-schizophrenic mental patients were more active than those from schizophrenics. There was no parallelism between the phytoactivity of the sera and their toxicity towards mice. The phytoactivity factor was insoluble in ether.

A disquieting factor in Jacobowsky's data is that the "phytoactivity index figures" were all much higher in the second series than in the first, even though the subgroups were in the same order in each series. The difference between the two series is unaccounted for, but probably depends on mould environmental factors or on the fact that different people did the two series.

Workers in Georgi's laboratory have recently reported that schizophrenic urine fractions were more toxic in mould sporulation tests than the corresponding fractions of normal urine (cf. 102).

D. *Production of Hyperglycaemia in Experimental Animals*

Interest in the role of glucose metabolism in schizophrenia stems from the empirical use of insulin coma as a therapeutic measure and the reports of abnormal glucose tolerance curves in some schizophrenics (3)

Hall *et al.* (50) initially reported on the occurrence of an anti-insulin (hyperglycaemic) factor in the blood serum of one schizophrenic woman. Meduna *et al.* (75) studied sera from 20 normals, 51 schizophrenics and 7

epileptics and found that an anti-insulin factor (AIF) was present in the blood of many schizophrenics which prevented the expected decrease of blood-sugar levels in rabbits following insulin injections. The average curve for blood-sugar decrease against time and the percentage of animals dying in hypoglycaemic coma was the same for epileptic and normal samples but "significantly less" for the schizophrenic samples. Each serum was tested in a separate animal because it was believed that the animal's response to insulin changed after one injection. The variation in individual blood-sugar loss curves was quite large in the schizophrenic group which was attributed to a great variation in the amount of AIF in schizophrenic blood.

Goldner and Ricketts (45) took exception to the conclusions of Meduna on the grounds that the variation between individual rabbits was so great as to render his data insignificant. Goldner and Ricketts used cross-over experiments in which the effect of schizophrenic blood plus insulin was compared with the effect of normal blood plus insulin and of physiological saline plus insulin in a single animal. Blood from 25 schizophrenics was compared with that from 20 staff and student members of the university. Both chronic and acute schizophrenics were included but all were seriously deranged at the time the blood was taken. Although Meduna *et al.* (75) had indicated that 9 such cross-over experiments in their hands gave results consistent with the data they reported, Goldner and Ricketts found no significant difference between the schizophrenic and the normal samples.

Harris (51) compared the effect in rabbits of schizophrenic serum plus insulin with a "standard" response to insulin alone. He found a significant anti-insulin effect in only 2 out of 7 schizophrenic bloods and there seemed to be no correlation with the clinical features.

In later work Meduna and his colleagues (74) studied urine rather than blood. Houssay (60) had reported that some anti-insulinic activity resided in normal urine and was increased in diabetic urine. Meduna used 24 hour urine samples from 21 normals and from 30 acute, untreated schizophrenics. The acidified urine was treated with kaolin, the filtered kaolin was eluted with ammonium hydroxide and the eluant treated with acetone to give a precipitate. The dry preparation from each 24 hour sample of urine was dissolved in about 10 ml. of water and injected intraperitoneally into a rabbit. With the control samples, the increase in blood sugar was generally maximal after one hour and averaged 37 per cent. of the fasting value; with the schizophrenic samples, the increase averaged 78 per cent. after one hour and 94 per cent. after two hours.

Moya *et al.* (82) repeated Meduna's work using intravenous injection of $2\frac{1}{2}$ hour aliquots of extracts prepared from 24 hour urine samples from 10 healthy normals, 11 acute, untreated schizophrenics and 6 non-schizophrenic mental patients. All were female. They confirmed Meduna on the difference between the blood sugar versus time curves for normal and schizophrenic groups. The curve for the non-schizophrenic group was not significantly different from the curve for the normals. Sixty per cent. of the rabbits receiving a schizophrenic extract died 3 to 24 hours after the injection. Preliminary symptoms were diarrhoea, cold ears, shallow rapid breathing and limpness; these were followed by convulsions, screaming and death. None of the animals receiving any of the normal or non-schizophrenic extracts died. The same extracts gave minimal effects in rats and no effects in mice or frogs (cf. Section C-1 (i)). On the basis of earlier work in the same laboratory the transient and slight hyperglycaemic action of the normal extracts (which can be inhibited by

adrenolytic agents (79)) may be due to the hypotensive agent, kallikrein (81); schizophrenic extracts seem to contain less kallikrein than do normal extracts and hence their more potent and longer-lasting hyperglycaemic effect must be due to some other agent (80).

Morgan and Pilgrim (78) reported isolation of a hyperglycaemic factor from urine by acidification to pH 4.5 with acetic acid followed by centrifugation; the active material is in the precipitate. The same or a similar factor can be isolated from urine by adsorption on to benzoic acid. Treatment of aliquots of a given urine sample by the two methods yielded materials of about equal activity. The hyperglycaemic activity was assayed by intraperitoneal injection into fasted rats; the results are variable from rat to rat. The concentration of the hyperglycaemic factor is greater by about 20-fold in the urine of acute schizophrenic patients than in that of normals. Moya and his group have confirmed this report using the Morgan and Pilgrim procedures (80); the active material is presumably different from that in extracts prepared by Meduna's procedure which give minimal effects in rats. Morgan and Pilgrim tentatively concluded that the hyperglycaemic factor is a non-dialysable protein or polypeptide. It is not readily adsorbed on to carbon, gives a positive ninhydrin, biuret and Molisch test, is not soluble in ethanol or ether, but is soluble in water except at pH 4-4.5.

Shiraishi (97) has also reported an abnormally high excretion of a hyperglycaemic factor by psychotic patients. He indicated that the amount excreted is approximately proportional to the severity of the symptoms and decreases with clinical improvement.

E. Effect on Enzyme Systems *in vitro*

The anti-proteolytic activity of serum is reportedly increased in schizophrenia, during epileptic stupor, in progressive paralysis, in uraemia and slightly in alcoholism. The extent of increase in schizophrenia correlates with the severity of the symptoms (88). The positive finding in uraemia is of some interest to those postulating a dysfunction in aromatic metabolism in schizophrenia since the mental symptoms in uraemia have been attributed by some to an accumulation of toxic aromatic compounds in the blood (7, 38).

Schizophrenic serum is said to have only about half the stimulating effect of normal or animal serum on glucose utilization by the isolated rat diaphragm and a depressing effect is noted with some samples. Sera obtained from patients who had benefited clinically from insulin coma treatment showed a normal stimulating action (113). It has been hypothesized that the inhibitory effect noted with schizophrenic serum is due to pituitary or adrenal hormones; signs of increased adrenal cortical function are often observed in active phases of schizophrenia (2). Haavaldsen *et al.* (102) recently reported on a substance in the alpha-globulin fraction of serum from many psychotics (especially schizophrenics) which inhibits glucose utilization by the isolated rat diaphragm.

Streifler and Kornbluth (103) studied the effect of serum from 49 acute schizophrenics and 20 chronic schizophrenics on glucose uptake by rat neural (retina) tissue *in vitro*. They found that schizophrenic blood serum inhibited the glucose uptake more than did normal serum. No difference in this inhibitory behaviour was noted between acute and chronic cases, between the different forms of schizophrenia and between the various age groups. Sera from female patients, however, proved generally more inhibitory than those obtained from males.

Hukuda (61) studied the rate of glucose metabolism by rat brain tissue in a medium containing serum from normals or from schizophrenics. The 10 schizophrenic sera studied showed a significant inhibition of glucose metabolism and particularly of the conversion to pyruvic acid, as compared with the 6 normal samples. There was no significant difference between male and female sera.

SUMMARY

The general conclusion which can be drawn from the numerous reports in the literature on the physiological effects of schizophrenic body fluids is that the subject is not yet on a firm experimental foundation. In many cases the experiments are inadequate to demonstrate the points they intend to prove. The cumulative weight of evidence would be impressive were it not for the disturbing number of conflicts amongst the various reports. Due allowance must be made of course for the unusual problems encountered in designing and executing convincing experiments, and encouragement can be taken from areas such as the toxicity of schizophrenic sera where there has been substantial confirmation of an initial report. Failure to obtain positive results on any particular test is not necessarily a setback to the psychotoxin concept of schizophrenia. Psychotoxic substances may exist but not be detected because of lack of suitability or insensitivity of the test, chemical alteration during the workup procedure or rapid detoxication by the bio-assay organism. The greater effect reported for taraxein in psychotics as compared to normal humans suggests that detoxication may be an important factor.

In each of the categories discussed, certain reports are particularly noteworthy. The work on experimental catatonia using schizophrenic bile is promising, but has not been followed up in recent years. Interest in bile may be encouraged by reports that LSD-25 or its metabolites are largely excreted through this medium (94). The recent work of the Tulane group on taraxein is dramatic, and, if confirmation can be obtained, considerable emphasis is bound to be placed on this lead. The method of rapid transfusions of schizophrenic blood should make confirmation possible in many centres.

In the area of non-catatonic C.N.S. effects, the experiments of Winter and Flataker stand out for methodology. They have been the first to apply to the problem a quantitative test which depends on a trained response.

Certain gratifying consistencies appear in the voluminous work on "abnormal toxic" effects of schizophrenic urine and serum. Although one must beware of assuming that various bio-assays are measuring the same or similar factors, many workers report such similar properties as the same percentage (60-70 per cent.) of "positive" tests with schizophrenic fluids and thermolability of the serum "factor".

The work of Rieder and others in Georgi's laboratory seems particularly convincing with regard to urine toxicity, since they have been at some pains to recognize and attempt to minimize the influence of artefacts. The report of Weichbrodt on the toxicity of schizophrenic serum to white mice has been confirmed by a number of workers. The experiments of Sjøvall stand out from a statistical point of view and are impressive because of his care in the selection of schizophrenic and control groups. Federoff and his co-workers have provided interesting information concerning the apparent properties of the serum factor toxic to cell cultures.

In summary, the reviewers strongly feel that a spirit of optimism should prevail in this field. In general, criticisms are directed against the quantity of work reported (which is often a factor not entirely within the control of the investigator) rather than any lack of promise. There are a number of exciting leads in each of the categories discussed which merit contemporary efforts at confirmation and extension. Furthermore, there is not a large negative literature in this subject as there is in so many others, and its potential importance probably justifies an expanded research effort.

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RESPONSE TO DRUGS AND PSYCHIATRY*

By

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FASHION is more deep-rooted than contemporary custom will allow, and the current enthusiasm for drug treatment in psychiatry is no evanescent fad. The Greeks made good use of the pharmacopœia, prescribing borage, buglosse, marigold, polypodie and epithyme for melancholy, more specifically recommending wormwood, centaury and pennyroll for the hypochondriac malady. Burton (a) discussing black hellebor, relates that its virtues were extolled by Galen, Pliny and Coelius Aurelianus, the mentally afflicted being sent to the Anticyrae, or to Phocis in Achaia to be purged, the plant growing there in abundance. Burton (b) describes how the Melangoga, or melancholy purging medicines, were classified as simple or compound, purging upwards or downwards. Asarum, brassivola, laurel, scilla, white hellebor, and antimony purged upwards, whereas polypodie, epithyme, black hellebor, lapis lazuli and aloes purged downwards. Sceptics throve then, no doubt, as they do now. Meryon (1861) mentions Asclepiades—dismissed by Pliny as an impudent quack, who contended that drugs injured the stomach and induced complaints more dangerous than those which they were intended to cure.

The slow-moving pace of ideas may be gauged from Culpeper, who lived in the mid-seventeenth century. He recommends melancholy thistle, germander, viper's bugloss, motherwort and burnet for melancholy. Of pills of fumitory, he warns, "It purges melancholy. Be not too busy with it I beseech you." He also condemns the cephalects or narcoticks as being inimical to both brain and senses.

Weyer was alive a hundred years earlier and is said by Zilboorg to have undermined the authority of the *Malleus Maleficarum* by suggesting that the behaviour of witches might be attributable to the effects of drugs (Zilboorg, 1941). Weyer studied response to drugs in the modern manner, being interested in affective and ideational changes after their administration. There remained a paucity of sophisticated literary descriptions of the psychological effects of drugs until, as Lindemann and Clarke (1952) point out, the evolution of romanticism in late eighteenth century England. Then followed Beddoes' *Consideration on the Medicinal Use of Factitious Airs* (Beddoes and Watt, 1796) Sir Humphry Davy's accounts of self-experiments with nitrous oxide (1800), Moreau de Tours' monograph "Du hachisch et de l'aliénation mentale" (1845) and Ludlow's *The Hasheesh Eater* (Ludlow, 1857). De Quincey's *Confessions of an English Opium-Eater* appeared in 1822. Later (according to Walton, 1938a) Gautier and Baudelaire belonged to a group who staged hashish debauches in the Hotel Pimodan of the Latin Quarter, Baudelaire later including his impressions in *Le Paradis Artificiel*. The systematic use of drugs for the production of "artificial psychoses" dates from the work of Kraepelin (1883). By the turn of the century, self-experiments with mescaline had been reported by Prentiss and Morgan (1896), Mitchell (1896) and Ellis (1897). William James dabbled with nitrous oxide and ether, suggesting that "they stimulate the

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mystical consciousness" so that "depth beyond depth of truth seems revealed to the inhaler" (James, 1910).

As for the treatment of mental illness, one finds Harley in 1869 recommending hemlock for mania, although bromides—long to remain the sheet-anchor of psychiatric treatment, were then in use. Barbitone was introduced in 1903 and the death knell of the bromides sounded by Barbour *et al.* in 1936. The following year, Putnam and Merritt (1937) reported that the anti-convulsive effects of a series of compounds (hydantoins) were not paralleled by their anaesthetic effect. This was a portent, for it had been assumed that a drug devoid of anaesthetic properties in large doses would also lack sedative or hypnotic effect. Still in 1937, Bovet and Staub described the first antihistamine and it was from these that some of the tranquillizing drugs were ultimately evolved.

Until then drugs had been classified as stimulant or depressant—labels of predictive merit when restricted to the effect of such substances on isolated organs. With respect to behaviour (and mental phenomena) the specificity of drug action cannot be delineated adequately in similar terms. If one is to define this action, it is essential to recognize participating variables other than those confined to the organism and drug. Such is the complex interplay even at the phenomenological level, that it invites Strauss's description (Strauss, 1953) of psychosomatic relations. "We have mind acting on mind as the efficient cause, mind acting on body, body acting on mind, and body acting on body as *causae agentes*"—only somewhere in this particular context, a chemical spoke has been inserted.

Symptomatic relief is the minimal goal of drug administration, and Modell (1955a) suggests that the "qualities which determine both the therapeutic endpoint and the toxic limitation of a drug are inherent in the substance itself, in the patient, and in the disease". If we are moving to a rational rather than an empirical basis for drug treatment of mental illness, then we must be cognizant of all factors determining drug response.

INDIVIDUAL VARIABILITY AND DRUG RESPONSE

Individual variation in drug response is well known, and Lewin (1931a) suggests that Galen was familiar with its existence. Ellis (1946) quotes from a footnote to the *Arabian Nights* illustrating this theme. "It is impossible to say how Indian Hemp—like opium, datura, ether and chloroform, will affect the nervous system on untried man. I have read a dozen descriptions of the results, from the highly imaginative Monte Cristo to the prose of prosaic travellers; and do not recognize that they are speaking of the same thing."

The precise formulation of this topic owes much to the ideas of Clark. As he remarks, "Variation may be regarded as one of the fundamental characteristics of living matter, and it is always found when the individuals of any population are measured in any way" (Clark, 1932a). The variation in response to drugs cannot therefore be a matter for surprise, and certain factors are recognized which are likely to determine variable response to drugs.

Clark (1932b) distinguishes between static and dynamic variants influencing drug response, an important differential criterion being that with the static variant there is a likelihood of the distribution of response approximating to the normal or bell-shaped type, whereas any form of distribution may occur with the dynamic variant. The symmetrical characteristic curves (curves relating dose of drug and incidence of some effect) occurring with mammals or other large organisms should approximate to the normal probability curve (Clark, 1932c). The normal probability curve is obtained when each measurement is

affected by a considerable number of independent factors, the curve expressing the chances of the various possible combinations of these factors. The curve does not show that all the factors have a symmetrical action, but a symmetrical curve is unlikely if a few of the factors are of greater weight than the remainder and have an asymmetrical action (Clark, 1932c). The classic study by Hanzlik (1913) demonstrated that the distribution in sensitivity to the toxic effects of salicylate was that of a normal curve. Newman (1947) showed the same thing for the distribution of failure at a co-ordination test after amylobarbitone sodium, as did Shagass and Naiman (1956) for the distribution of the sedation threshold.

Because a distribution follows some simple statistical parameter, it would be unwise to assume that response was determined in as simple a fashion. Clark showed that response to drugs at the simple isolated organ level is incapable of a single interpretation and he mentions three formulae each satisfactory for objectifying the identical set of dose (x) and response (y) data. These are (1) the hyperbola, $kx = \frac{y}{100} - y$; (2) the exponential, $k = \log(ax + 1)$ and (3) the parabola, $kx = y$ (Clark, 1932d). He cautions (Clark, 1932e) "highly complex systems may provide the simplest quantitative relations between dosage and action of drugs, and the most probable reason for any such apparent simplicity is that a large number of variables are present but mutually cancel each other". This should be borne in mind when one attempts to isolate variables affecting human response to drugs.

Variables determining drug response include body weight, surface area, age, and sex. Orton, 1957, suggests that sex differences may determine response to tranquilizers, and that methylpentynol helps females but not males. Marquis *et al.*, 1957 found meprobamate more efficacious in reducing anxiety in females than males. Other variables may be haemoglobin levels, hepatic and renal function. Of more interest to the psychiatrist is the alleged relation between personality and reaction to drugs. Kennedy (1957) alluded to the lack of work in this field and it is salutary to find Jonathan Hutchinson (1884) remarking "Our forefathers, who knew far less about the details of pathology than we do, attached far more importance to such matters as temperament and diathesis. They were accustomed to prescribe for a man's temperament . . .". Modell enigmatically concurs—"There must indeed be constitutional features which enter into the response of a particular patient to a particular drug" (Modell, 1955b).

Burton (c) quotes from Dioscorides that "white hellebor should not be taken by old men, youths, such as are weaklings, nice or effeminate, or fear strangling". In 1858, Kidd wrote that hysterical or nervous young women require greater quantities of inhalants to produce anaesthesia. In more modern vogue, McDougall (1929) suggested that the marked extraverted personality was susceptible to the influence of alcohol, whereas the introvert was more resistant. Lewin (1931b) probably implies the same thing when he held that each person has his own "toxic equation". Sheldon classified body type into three components (endomorph, mesomorph, and ectomorph) each correlating with a group of traits described as viscerotonia, somatonia, and cerebrotonia. He asserts (1942a) that cerebrotonics are resistant to alcohol and cerebral depressants. Viscerotonics respond well to alcohol, with no sense of dizziness, drowsiness or fatigue (Sheldon, 1942b) but somatonics are susceptible to central nervous system depressants (Sheldon, 1942c).

Vernon, Fleming, Eysenck and Cattell have advocated a dimensional approach to personality assessment, and Vernon, like Eysenck, believes that personality can be resolved into orthogonal dimensions along which individuals can be measured. Eysenck (1953) termed two of these dimensions "Neuroticism-Normality" and "Extraversion-Introversion" and tests were devised for

measuring these quantities (Eysenck, 1955, 1956, 1957a). Assuming that the tests do objectify these aspects of personality then it should be simple to ascertain whether extraverts are susceptible and introverts resistant to central nervous depressants. Eysenck (1957b) considers that "The most important variable in predicting the effects of a drug, and in prescribing the particular dosage required for a specific purpose, would be the excitation-inhibition ratio (in the Pavlovian sense) obtaining within the particular person concerned."

Work along these lines was initiated by Shagass. Shagass (1954) described a technique for estimating the sedation threshold. This purports to be an objective pharmacological measurement deriving from electro-encephalographic and speech changes concurrent with the intravenous injection on a dose:weight basis of amylobarbitone sodium. Shagass concluded initially that the sedation threshold is an index both of anxiety, and what he designates impairment of ego-function. Shagass and Naiman (1956) elaborated this argument, contending that extraverted individuals (hysterics and psychopathic personalities) have a low sedation threshold, whereas introverted subjects (patients with anxiety states, obsessional or depressive illnesses) have a high sedation threshold. As the alterations in the electro-encephalogram and speech (increase in 15-30 cycles-per-second activity recorded over the frontal and central regions of the brain and the onset of dysarthria) taken as the end-points for the sedation threshold are those indicating intoxication by the particular drug, it follows that extraverted individuals would manifest susceptibility to central nervous system depressants while introverted patients would be resistant.

Justifiable criticism of Shagass's work has been forthcoming (Thorpe and Barker, 1957; Ackner, 1958; Pampiglione, 1958) the objections being to the difficulty of determining accurately the specified end-points. Pampiglione (1958) could demonstrate a definite sedation threshold in only one-third of 58 patients. He concluded, "The epiphenomenon of anxiety does not bear recognizable relationship to the patient's resistance to a sedative of the kind employed."

Apart from susceptibility there are other qualities of drug response. Kornetsky and Humphries (1957) found that subjects with high scores on the Depression and Psychoaesthesia scales of the Minnesota Multiphasic Personality Inventory responded with maximum subjective changes after chlorpromazine, meperidine, secobarbitone or lysergic acid diethylamide (LSD). It was surmised that there are reactors and non-reactors to drugs of whom the reactors are likely to be individuals who are depressed and/or likely to experience unreasonable fears, as well as to over-respond to environmental stimuli. Felsing *et al.*, 1955 believed that subjects with abnormal personalities responded atypically to amphetamine and morphine. By degrees then the emphasis is changing from drug reaction and personality to psychiatric illness and drug response.

Goodman and Gilman (1955) mention that "hang-over" the day after barbiturate administration is indicative of idiosyncrasy to the drug, and that this is prone to occur from small doses of the substance in neurotic patients. Dickel and Dixon (1957) recounted their experience of tranquillizing drugs prescribed to 8,200 individuals with psychosomatic illnesses. They found that 4-5 per cent. of their population (328 cases) developed physical disturbances during treatment, while over 30 per cent. (2,527 subjects) showed behavioural changes or striking alterations in the mental state. Anxious patients became so depressed as to attempt or commit suicide; calm people became hypomanic, others seemed more anxious, and some exhibited amoral behaviour alien to their previous character. Dickel and Dixon linked the presence of anxiety with adverse response to drugs.

Dickel and Dixon's conclusions are novel in that they point to the adverse effects of drugs hitherto considered most suitable for alleviating anxiety, and although Kornetsky *et al.*, 1957 have indicated a possible dichotomy between the objective and subjective effects of a drug making it impossible to predict accurately the extent of one from the other, the fact that so many anxious individuals developed physical signs with tranquillizing drugs reflects doubt on Shagass's contention that anxiety can be equated with a high sedation threshold and that only one personality dimension (extraversion-introversion) is linked with drug susceptibility. It should be a simple matter to elucidate to what extent say the dimension of Neuroticism-Normality contributes, if at all, to subjective and/or objective response to drugs.

The type of psychiatric illness also determines the dose of drug required. Variation in reserpine requirements for neurotic and psychotic populations was noted by Kline (1956a) who suggests that this may be due to some fundamental metabolic difference. Kline (1956b) also mentions that the scatter in reserpine dosage is greater among neurotics than among psychotics. Moore and Martin (1957) who used reserpine, epitomized their conclusions thus, "Each case must be treated individually, and dosage modified according to the side-effects and the mental state." Rees and Lambert (1955) found considerable variation in the optimum dose of chlorpromazine for anxiety states, while for psychotics, Lieberman and Vaughan (1956) allude to the variability in response to the drug. Mayer-Gross *et al.*, 1953 mention that schizophrenics have greater tolerance to LSD than normals, while Lindemann (1934) found distinct differences between schizophrenic and neurotic individuals in their reaction to drugs, Sargant and Slater (1954a) point to the wide range of response to amphetamine sulphate, and it is known that some psychopathic patients may receive continued large doses of the substance without sleep disturbance (Hill, 1947) as can children with behaviour disorders associated with electroencephalographic abnormalities (Sargant and Slater, 1954b). Anomalous response to amphetamine was encountered by Cameron and Kasanin (1941) in two patients whose sleep improved after the drug.

Sargant seems always to have been interested in variability of response to drugs, noting of soldiers with acute psychiatric illness that some respond well, but others by worsening of their symptoms, to heavy sedation (Sargant and Shorvon, 1945). He comments (Sargant, 1956) that it is the hysterical subject who is likely to become ataxic after less than 1 gr. of phenobarbitone daily, whereas the strong constitutionally aggressive patient may get only a modicum of sedation from half-a-bottle of whisky. Sargant subscribes for explanation to Pavlov's work (1927) demonstrating that dogs with dissimilar constitutions require different doses of bromide to restore nervous stability. Pavlov's "strong excitatory dogs" which easily developed stable conditioned salivary responses (said to correspond with the aggressive subject) needed eight times more bromide than a dog of the same body weight but with a "weak inhibitory" constitution and which developed conditioned salivary responses with difficulty (apparently corresponding to the hysterical patient).

This is an argument capable of verification. Franks (1957) demonstrated a relation between the conditioned eyeblink response and personality dimensions. He showed that conditionability was linked with introversion-extraversion (extraverts condition badly, introverts condition well) but was unrelated to neuroticism. Franks extended his work to the clinical field, discovering that neurotics of the introverted type (anxiety and neurotic depressive reactions) condition easily, but extraverted neurotics (conversion hysteria)

condition with difficulty (Franks, 1956). Using such an approach, supplemented by personality questionnaires, one should be able to quantify personality attributes in terms of susceptibility. The difficulty about personality or questionnaire studies applied to psychiatric patients is that test-retest data rarely exist for patients who once ill have since recovered. An atypical response (susceptibility) to the drug may therefore be related primarily to the illness or to the basic personality. The other objection in this instance, is of grafting conclusions from animal work on to humans, and that the factors involved in the development of conditioned salivary responses may bear no relation to those implicated in the development of the conditioned eyeblink response.

Other factors contribute to drug response. Hill *et al.*, 1955 showed that motivation was an important variable, for the effects of morphine and pentobarbitone on reaction time could be altered by penalizing slow responses with an electric shock. They amplified this work (Hill *et al.*, 1957) this time substituting pleasant rather than unpleasant incentives for the testee. They found that morphine and phenobarbitone might retard, facilitate, or have no effect on the performance, depending on the incentive.

It has also been construed that individuals with a predisposition to mental disorder may react abnormally to medication, a special instance being the response to ACTH and cortisone (Evans and Rackemann, 1952; Cope, 1953). In a typical searching article, Lewis and Fleminger (1954) confound these strictures, concluding that predisposition to develop untoward mental symptoms with ACTH or cortisone could not be assumed in patients with unstable neurotic personality or a history of mental illness.

So far we have confined ourselves mainly to the patient himself but there are other external variables influencing his reaction to drugs. As Foulkes and Anthony (1957) remark, "Even in the most isolated and insulated conditions, in certain kinds of stupor and catatonic episodes, human beings do retain some relationship with the environment." Some intriguing facts have been thrown up by animal investigations. Chance (1946) noted that mice given amphetamine evinced little response when kept separately, but if grouped together, then aberrations of behaviour appeared. It seemed that the activity of one mouse excited another, eventually communicating itself until all the animals were hyperactive. It was found that the LD 50 of amphetamine for solitary mice was 90 mg./kg. but only 7 mg./kg/ when groups of mice were kept in a confined space. Chance (1956) also reported that the response of rats to follicle stimulating hormone was modified by the environment. Similarly, Chen (1954) noted that the effect of a hypnotic in rats and dogs varied with the degree of isolation of the animal. Woolley found, after the administration of LSD to mice to make them walk backwards, that the effect could be abolished by injecting carbamylcholine into the mouse's lateral cerebral ventricle. It was vital to keep the mice caged separately for at least a day before the experiments, otherwise the drug either failed to abolish the LSD effect or there was a greater scatter in the amount of drug required to do so (Woolley, 1955). Woolley attributed the discrepancy to the fact that "excitement engendered by suddenly creating a new social group may have influenced the production of endogenous acetylcholine".

Lindemann and Clarke (1952) say this in respect of human reaction to drugs, "Situational factors, both those in the patient's life and those in the immediate experimental situation, may significantly alter the prospect that a given response will occur." Rathod (1958) indicated that environmental factors play a large part in the apparent effectiveness of tranquillizers in the treatment of disturbed patients. Sabshin and Eisen (1957) listed a number of social factors

likely to determine response to drugs. These included (1) the attitude of the psychiatrist towards such treatment, (2) the quantity and quality of the ward personnel, (3) the physical construction of the nursing unit, the number of patients per room, the size of the unit, and the available recreational space, and (4) the type of patient being treated together with the degree of disturbance on the ward.

Lindemann and Malamud (1934) suggested that "each drug has certain specific characteristics, but these are quite closely related to the conditions present at the time when these specific effects are produced". Grace (1954) writes, "In evaluating a drug one needs to know not only the nature of the drug but also the status of the individual at the time the drug is given." An amplified statement comes from Savage (1956). He considers that among the factors determining response to LSD, the personality of the subject, his psychological defences and psychopathology are important. Individuals with good defences evince little reaction; on the other hand, pre-schizophrenics may develop a full unfolding of their latent psychosis. The mental set of the individual at the time of the experiment also contributes, as well as the reasons for taking the drug. Individuals with moderate anxiety tend to minimize or deny the onset of the LSD experience, but subjects with overwhelming anxiety undergo an intense reaction. (Denber and Merlis, 1955 suggested that anxiety seemed to be the determinant of response to mescaline, its presence being associated with a florid mescaline picture, its absence being correlated with apathy and an absence of symptoms.) Neurotic motivations for receiving the drug portend a severe response because of the guilt engendered in acquiescing to hostile or dependent wishes. A subject who takes the drug when anxiety free, and on another occasion suffering from marked anxiety, will then have a more severe reaction. The presence of another person minimizes the toxic response; loneliness exacerbates it.

Before leaving the matter of individual drug response, reference must be made to the "placebo responder", recent interest being sparked off by Wolf (1950). Apart from the fact that previously this has been an unsuspected quantity in drug response, either enhancing or subtracting from drug effects, the practical importance of the placebo responder is that a 15-58 per cent. favourable response may follow its administration (Beecher, 1955). Tibbetts and Hawkings (1956) claim that the majority of "novel physical treatments" such as carbon dioxide inhalation and intravenous acetylcholine "are likely to prove placebos".

SYMPTOMATIC RESPONSE TO DRUGS

The personal equation is encountered in the variability of symptomatic response to drugs. Walton (1938b) says this of response to marihuana, "Some individuals seem to be very limited in the sensations they experience, whereas others are subject to an almost infinite variety of emotional and physiologic reactions." Ayd (1957) mentions that patients who complain of depersonalization or feelings of unreality are often made temporarily worse by chlorpromazine. Moreover, obsessionals and hysterics are prone to react to the side-effects of tranquillizers which they then make the subject of their complaints. Kinross-Wright (1956) noted that neurotics became more anxious with chlorpromazine, and neurotic depressives more depressed. Salisbury and Hare (1957) remark that central nervous stimulants are not of benefit in schizophrenia. Sargant and Slater (1954c) are more dogmatic: "Benzedrine, in the schizophrenic, may precipitate an unexpected outburst of excitement, or provide the initiative to carry out a murderous attack inspired by the patient's delusions." Esoteric behavioural responses have been ascribed to the tranquillizers. A house-

breaking and motor-cycle fatality occurred after two youths had each taken between 0.75–1.0 g. of methylpentynol (*Lancet*, 1955) and Pratap (1956) refers to motor accidents and to individuals submitting to seduction by strangers after drugs. As Weatherall (1957) interpolates "Controlled studies are particularly lacking in these circumstances and it is very uncertain whether some of the events described would not have occurred without the intervention of a tranquillizer." Leiberman and Vaughan (1957) remarked on the indirect effect of chlorpromazine on the behaviour of chronic psychotics: "When the most disturbed members of the community become quiet and co-operative, other patients become more settled and socially improved."

Most drug-induced symptoms are non-specific in character, probably on account of the limited number of behavioural, affective, and ideational responses available, the brain (personality) acting, in the Sherringtonian sense, as the final common path for their determination. It may seem facetious to emphasize the non-specific symptomatic effect of drugs, but Curran after experience with bromide intoxication (Curran, 1938) believed that he could distinguish features specific to drug intoxications, asserting that paraphasia and hallucinations at a distance were the hall-marks of bromide deliria (Curran, 1944). There seem to be no logical grounds for assuming such symptomatological exclusiveness, and Mayer-Gross *et al.* (1954a) allude to paraphasia in delirium tremens. Stengel and Mayer-Gross (1945) refer to paraphasic disturbances during recovery from hypoglycaemia. They noted, in deference to Paterson's criticism of a quantitative one-dimensional idea of consciousness (Paterson, 1944) that there were at least four levels of consciousness during hypoglycaemic arousal, paraphasia being associated with least clouding of consciousness. Paraphasia has also been encountered with methylpentynol intoxications (Marley, 1955; Marley and Chambers, 1956). Weinstein and Kahn regard paraphasia as one manifestation of the language of denial. They were able to reproduce symptoms of denial (anosognosia) by barbiturate administration in patients with pre-existing brain damage. It seemed that the type of denial expressed was predominately determined by the character of the premorbid personality (Weinstein and Kahn, 1955a). Paraphasia was evident only when the patients were required to name objects associated either with their illness or some personal problem (Weinstein and Kahn, 1955b). They concluded that motivation to deny illness and incapacity exists in everyone and that the phenomena of verbal denial such as disorientation, reduplication, and paraphasia, are modes of adaptation to stress rather than individual deficits (Weinstein and Kahn, 1955c). It is intriguing then that Teitelbaum (1941) demonstrated visuo-spatial disorders and anomalies of body image induced by hypnotic suggestion, as was shown for Gerstman's syndrome by Stanton (1954).

Affective changes follow the administration of many drugs, being categorized as non-specific by Cleghorn (1952). Elevation of mood is conventionally associated with the administration of dextro-amphetamine sulphate (Peoples and Guttman, 1936; Guttman and Sargant, 1937) and more recently of "Preludin" (Randell, 1957) and "Meratran" (Begg and Reid, 1956; Fullerton, 1956). Depressive mood changes may also follow the ingestion of cerebral stimulants (Cleghorn, 1952; O'Flanagan and Taylor, 1950; Shorvon, 1945; Bethell, 1957; Connell, 1957a). Both elation and depression may complicate barbiturate therapy (Curran, 1944; Stafford-Clark, 1957). Walton (1938c) reports: "Euphoria or apprehension may follow marihuana. The extent to which these effects occur is thought by some to be due to the preliminary state of the mind." Anderson and Rawnsley (1954) noted a differential effect of

LSD on mood. They say, "On occasions the drug may seem to underscore the clinical picture, e.g. depression may become enhanced, but next day the same dose may elicit a state of euphoria in the selfsame patient". Mood changes are common with the tranquilizers, occurring with methylpentynol (Glatt, 1955; Marley and Chambers, 1956) meprobamate (Hollister *et al.*, 1957), chlorpromazine (Ayd, 1955) and reserpine (Freiss, 1954; Wilkins, 1954; Wallace, 1955; Platt and Sears, 1956). Kline (1956c) suggests that depression associated with reserpine therapy is not a specific drug effect but is due to the breakdown of the individual's ego defences. In a controlled trial with benactyzine, Hargreaves *et al.* (1957) found that five patients became more depressed. However, at the conclusion of the investigation, the mean "depression scores" for patients receiving the drug and those taking inert tablets were identical.

Wikler (1952a) points out that euphoria is a term with a variety of meanings, being conventionally defined as a sense of unusual well-being. However, what constitutes well-being for one person may differ radically from that in another, or for the same person at different times in different situations. Thus, a post-addict may vomit and appear pale after morphine, but reports of feeling unusually well. The euphoric state then seems to be related to reduction of pain, hunger and sexual urges. This type of person does not experience unusual well-being after barbiturates except in doses which produce gross intoxication, when euphoria seems to be associated with loss of self-control, "acting-out" of hostility and sexual aggressiveness, and impairment of the sensorium with anosognosia or denial of illness. Euphoria in non-addicts also describes phenomena with a wide spectrum of meaning. Wikler concludes that euphoria and dysphoria are related each to the other in that they usually crystallize out in the presence of an impaired sensorium. Many drugs may produce euphoria in post-addicts and normal individuals (Wikler, 1952b). He cites alcohol, cocaine, cannabis, antihistamines, and large quantities of coca-cola and coffee. Mood changes are predominant in association with ACTH or cortisone treatment (Pearson and Eliel, 1950; Galdston *et al.*, 1951; Kirsner and Palmer, 1954) and with isonicotinyl hydrazide (Robitzek *et al.*, 1952). It would appear then that the terms "stimulant" or "depressant" outside the isolated organ context are not specific enough to be rigidly adhered to. In the human frame of reference such drugs may produce antipodal mood effects even in the same person.

Alteration in appreciation of the self, or what Schilder (1953a) terms "ego-experience" may follow administration of drugs. It would be a mistake to dismiss these changes as merely side-effects. Alteration in subjective time may occur with cerebral stimulants or depressants. Subjective time may appear to pass faster or slower, even culminating in an apparent standstill or timelessness. The rate of passage of subjective time may then be related to mood changes, or even, due to the effect of the drug, to alteration of body temperature. Pieron, who extended to temporal phenomena the idea relating chemical velocity to temperature (Arrhenius's equation), found that subjective time passed more rapidly with a lowered temperature, whereas with an increase of temperature, subjective time appeared to decelerate. Like other aspects of drug action, alteration of subjective time must be gauged against the background of the individual. "Psychological time is only an aspect of ourselves" (Carrell, 1948) and as Chessick affirms, "Time perception may be disturbed like any other perception, the experience of time being interwoven with emotional factors and the actual biological situation of the percipient" (Chessick, 1956). Alterations of subjective time can be elicited after many drugs, the most usual being LSD

and mescaline. Hughlings Jackson (1952) suggested that this "prolongation of time was the outcome of shallow dissolution of the highest centres and could be interpreted as a more rapid succession of a Time Constant peculiar to each individual". In fact "the dissolution of the highest centres" may not be obvious by ordinary clinical tests.

Disorders of subjective time are frequently associated with depersonalization. Again to quote Schilder, "Cases of depersonalization whose total experience is splintered, all have an altered perception of time" (Schilder, 1953b). Mayer-Gross (1935) regarded symptoms of depersonalization as of non-specific origin although resulting from a "preformed functional response of the brain". That depersonalization should occur in drug intoxications is not surprising for "Anything which leads to an altered state of consciousness or interferes with the final associative integration is bound to result in some changes in the relationship of the individual to his world, his body or his own psychic functioning" (Ackner, 1954). However, depersonalization (as well as subjective time disorder) may follow the administration of placebos. (Tyler, 1947 reports even hallucinations after administration of placebos.) For this reason, one is driven to enquiring what the symptom or drug represents to the patient, the psychodynamic basis being as applicable for drugs as for placebos. This personal and overvalued attitude towards drugs may be epitomized by Fenichel's comment, "Drug prescriptions, in so far as the patient believes that 'good stuffs' may neutralize 'bad stuffs' serve as a kind of artificial paranoia" (Fenichel, 1946).

The presence of illusions and hallucinations is also regarded as evidence of abnormal drug response, being almost constantly elicited by the "hallucinogens". Again it would be unwise to underestimate the personal element, and Ardis and McKellar (1956) comment, "With both mescaline and hypnagogic images, as with dreams, personal interests together with possibly deeper psychodynamics, seem to play a part in determining content." The lay writer Ward (1957) believes that LSD and mescaline "merely reveal the content of the subject's psychological being". Noyes (1951a) remarks that not only are illusions likely to be determined by the prevailing trend of the patient's pre-occupations, but that the mental material which is externalized in the form of hallucinations, is of a most intimate, subjective, and personal nature (Noyes, 1951b). Hadfield (1954) suggests that the hallucinations of fevers relate to repressed emotional experiences with which the subject's mind was already preoccupied.

Disorientation has already been briefly referred to in terms of denial of illness. Levin (1956) distinguishes between delirious and paranoid disorientation and that with organic brain disease. He considers the disorientation of toxic delirium the easiest to understand, being in Hughlings Jackson's words a "reduction to a more automatic condition" (Levin, 1936), the tendency being for the patient to mistake "unfamiliar for familiar" (Levin, 1945). Levin (1951) also formulated the entity of "partial delirium", which depended on the fact that orientation for time, being an abstract concept, was more vulnerable and consequently more likely to be upset than either orientation for place or person. In fact, one may not even find "partial delirium" and Connell (1957b) noted absence of disorientation as a symptom of amphetamine intoxications.

The extreme response to drugs is either stupor or delirium. Hoch (1921) comments that "If a stupor be a reaction type, its laws must be psychological." Bleuler (1944) considered delirium one of the basic modes of cerebral reaction. Mayer-Gross (1951) held that "Differences in symptoms found with various drugs have been attributed to differences in the personality of the intoxicated.

This may be true for an early stage or for cases of mild intoxication. The delirious picture, on the other hand, is probably common to all intoxicants when their effect is most severe. Between the two ends of the scale is a stage in which probably each drug shows certain special features." Hoch (1906) observed that excessive amounts of sedatives produce indistinguishable deliria, while earlier still, Anstie (1864a) likened alcohol to chloroform intoxication. He also quotes a case of belladonna poisoning with features resembling delirium tremens (Anstie, 1864b). Knauer and Maloney (1913) noted similarities between mescaline poisoning and alcohol intoxication. Leake (1957) discussing the hallucinogens, states that although these are related to the indoles, the same kinds of effects may be caused by ethanol and cannabis which are unrelated to the indoles. Mayer-Gross *et al.*, 1954b suggest that the patterns of reaction to intoxicants are best understood from a study of the effect of anoxia. They consider oxygen deficiency an important chapter in pharmacological psychiatry. MacFarland (1939) evidently believed this, demonstrating that the same degree of oxygen deprivation may elicit a variety of behaviour responses in normal subjects.

An important contribution came from Wolff and Curran (1935). After analysing deliria due to 27 different agents, they concur with Bonhoeffer and Krisch that there is no single aspect of a delirious reaction attributable to one substance alone. They point out that although Lewin described a number of specific reactions to drugs, he cites no information regarding the environmental setting or the personality status of the patients involved. As for the content of the deliria, they find that the more timid, shy, or insecure the patient, the greater fear they show, whereas those with the greatest self-confidence react with least. The age, sex, and intellectual endowment of the subject are reflected in the content. Persons with marked characteristics preserve these in their deliria—the bombastic, the pathologically suspicious, and those with obsessional trends manifesting these traits but in an intense form. Jellinek (1942) is more sceptical. "Although personality characteristics break through into the picture of intoxication, it is not possible to construct a law of relation between types of alcohol reaction and personality."

VALIDATION OF THE EFFECT OF DRUGS USED IN PSYCHIATRY

Lewis (1958) says this in his Bradshaw lecture: "The cause of a mental illness is affected by so many factors, within the patient and his environment, it is so subject to unforeseen turns of fortune that a change cannot safely be attributed to therapeutic intervention unless it is frequently and regularly produced or comes prompt on the heels of the treatment. This . . . can be overcome by rigorous trials using, as controls, patients closely comparable to those who are treated. But matching psychiatric groups for this purpose is a daunting business, since they should be at least alike in the distribution of sex, age, intelligence, duration of illness, form and severity of illness, and previous illness."

Finney (1955) has declared as false the notion that investigations can be conducted statistically or non-statistically at the whim of the investigator. Claude Bernard, who showed no relish for the statistical method, would probably have granted it essential for the validation of drugs used in psychiatry. As he says, "Statistics therefore apply only to cases in which the cause of the facts is still indeterminate" (Bernard, 1949). Nevertheless, reluctance to employ sound methodology persists in spite of the fact that a statistical assessment implies ultimate economy of effort. As Hume (1957) remarks, "The theory of statistics enables an experiment to be planned so that the maximum information may be obtained from a limited number of observations required for a given conclusion."

The procedure adopted in 15 drug trials with reference to psychiatry and reported in British journals will now be analysed. The list is not intended to be comprehensive although in each instance control groups were employed (see Table I).

Selection of Patients. The patients were neurotics or psychotics, the neurotics being selected on the basis of symptomatology—usually the presence of anxiety and tension (Trials 2, 6, 7, 11, 13). In the case of psychotics, either one category of diagnosis was studied, e.g. schizophrenia (Trials 3, 5, 14, 15) or groups of mixed psychotics (Trials 1, 8, 9). In trials 10 and 12, both neurotics and psychotics seem to have been included, while in Trial 4 no diagnosis is specified, patients being selected because they were “restless, agitated, and showed psychomotor excitement”. It is obviously an advantage to study a group as homogeneous as possible.

Sex. No sex was specified in Trials 10, 12 and 13. One sex was studied in Trials 3, 4, 5, 9, 15 and both sexes in Trials 1, 2, 6–8, 11, 14. In view of the possible differential response to tranquillizers (Orton, 1957; Marquis *et al.*, 1957) it would seem desirable to include patients of both sexes.

Type of Trial. It is axiomatic that drug trials should allow for proper controls, or, as Gaddum (1954) indicates, errors of the first order will result. Adequate control may be obtained by allotting the patients (preferably randomly) to the placebo and drug group (Trials 3, 4, 6, 7, 10, 11, 13, 15). In three trials, patients were more exactly matched for “neuroticism” (Trial 7) age, sex and aggressive behaviour (Trial 8) and suggestibility (Trial 11). Thorpe and Baker (Trial 15) resorted to the device of matching at the end of the experiment by use of analysis of covariance. A more sensitive method was utilized by Rees and Lambert (Trial 2) the drug and placebo being alternated and the groups split up so that a cross-over design could be applied. They say, “The method of using the patient as his own control has much to recommend it, especially if such methods as the double blind and sequence control procedures are utilized enabling the effect of suggestion to be ascertained and also the control of factors unrelated to the pharmacological effects of the drug.” They echo the sentiments of Reid (1954): “Since each drug is tested consecutively on the same patient, variations due to differences in responsiveness between patients is eliminated.” The “self-controlled” technique was used also in Trials 1, 5, 8, 9, 12, 14.

Hill (1951) has emphasized the difficulties in interpreting the results of trials in which the precaution of randomization has not been followed. Where drugs (or placebo and drug) follow one another in randomized sequence there may be a “residual” or “carry-over” effect from one drug to the next. This can be overcome by use of a special Latin Square for randomization, as elaborated by Williams (1949). The majority of the above investigators adhered to the “Double-blind” type of trial which according to Modell (1955c) is essential for studying the effects of drugs on symptoms.

Dose. Ideally at least two dose levels of the drug should be alternated with placebo, as it increases the probability that at least one of the doses will fall in the steepest part of the dose-response curve for the group. If three dose levels are employed then a dose-response regression and relative potencies of the drugs can be obtained. If two or more dose levels are used then they should be related geometrically to each other, as geometric increments or decrements of dose are associated with arithmetic increase or decrease of drug response. Drugs were given at two dose levels in Trials 1, 3, 8 and 15.

Analysis of Results. Although all the above drug trials were controlled,

trial	author	drug	dose per day	diagnosis	sex	method	assessment	statistics	results
1	Elkes and Elkes, 1954	Chlorpromazine	75-300 mg.	Chronic psychotics Mixed diagnoses	12 M 15 F	Blind and self-controlled	By doctor and nurse observers	No statistics	Drug may have its place in the management of the chronically overactive psychotic.
2	Rees and Lambert, 1955	Chlorpromazine	75 mg.	Anxiety states	49 M 101 F	Double-blind self-controlled	By patients' subjective responses and by clinical assessment	Critical ratio	Chlorpromazine significantly better than placebo for relief of anxiety.
3	Mitchell, 1956	Chlorpromazine	150 and 300 mg.	Chronic schizophrenia	36 M	Double-blind control group	Effect of drug on number of defined aggressive incidents	Statistical analysis, although no statistics given	Drug had no effect on aggressive outbursts.
4	Vaughan, Lieberman and Cook, 1955	Chlorpromazine	75-450 mg.	Diagnosis not specified	48 F	Double-blind control group	By clinical assessment	χ^2	Drug highly effective in control of agitation and excitement.
5	Salisbury and Hare, 1957	Chlorpromazine and Ritalin	150 mg. 30 and 60 mg.	Chronic schizophrenia	48 M	Double-blind self-controlled	Rating by doctors and nurses	P values	Significant improvement in behaviour with chlorpromazine Ritalin no better than placebo.
6	Davies and Shepherd, 1955	Reserpine	1 mg.	Neurotics with anxiety and depression.	54 M and F	Double-blind control group	By doctors and patients with questionnaire	χ^2	Reserpine treated patients significantly benefited as compared to placebo group.
7	Ferguson, 1956	Reserpine	1-3 mg.	Neurotics with anxiety and tension	16 F 24 M	Double-blind control group (matched)	By clinical evaluation and 10 objective tests for anxiety	t tests	Effect of drug disappearing.
8	Wing, 1956	Reserpine	6 and 15 mg.	Chronic psychotics Mixed diagnoses	17 M 34 F	Double-blind Self-controlled in part	Rating by doctor, nurse and sociologist	t tests, χ^2 , exact binomial product method. Analysis of variance	Reserpine decreased tension and increased sociability in aggressive and overactive chronic psychotics.
9	Fullerton, 1956	"Meratran"	6 mg.	Chronic psychotics Mixed diagnoses	40 M	Double-blind Self-controlled	Doctor and nurse clinical assessment	No statistics	Drug benefited patients with depressive features without anxiety or agitation.
10	Bocknei, 1957	Methylpentynol	Up to 2 g.	Neurotics and psychotics	87 SNS	Control group	Clinical assessment	No statistics	Two-thirds of patients with anxiety improved.
11	Heller <i>et al.</i> , 1957	Meprobamate	1-6 g.	Neurotics—anxiety and tension	16 M 16 F	Matched control group	Clinical rating by doctor. Also questionnaire	t tests	Tension relieved by meprobamate.
12	Folkson, 1957	Meprobamate	0-8-1-6 g.	Neurotics and psychotics	23 SNS	Double-blind self-controlled	Clinical rating	No statistics	Drug effect disappearing.
13	Hargreaves <i>et al.</i> , 1957	Benactyzene	3-8 mg.	Anxiety states less than 4 years duration	32 SNS	Double-blind Control group	Clinical rating by 3 doctors	t tests	Patients receiving drug showed greater improvement than those receiving placebo.
14	Gray and Forrest, 1958	Azacyclonal	60 mg.	Chronic schizophrenia	20 M 20 F	Double-blind Self-controlled	Clinical assessment	χ^2	Apparent superiority of placebo over drug
15	Thorpe and Baker, 1956	Chlorpromazine and "Pacatal"	Both at 150 and 300 mg.	Chronic schizophrenia	48 F	Blind. Matched control group	Doctor and nurse assessment, Rating scales	Analysis of Covariance	At dose 300 mg. daily chlorpromazine superior to Pacatal in relief of "deteriora-

in some instances there is no mention of the statistical criteria used; in others χ^2 or the "t" test were employed. Where quantitative data are available, analysis with χ^2 means a loss of potential information, as this is a test strictly for homogeneity of the experimental population. As indicated earlier, there are a large number of factors contributing to variability of response to drugs and use of the "t" test which only distinguishes between intergroup differences, precludes any elucidation but the summed effect of these variables. With analysis of variance, "The separation of variances ascribable to one group of causes from the variance ascribable to other groups" can be achieved (Fisher, 1950). Analysis of variance may therefore "be regarded as an extension of the 't' test appropriate to cases where more than two variables are to be compared" (Fisher, 1953). Wing (1956) used analysis of variance for validating part of her results. Thorpe and Baker (1956) relied upon analysis of covariance (an extension of analysis of variance). This allows one "to adjust experimental comparisons for extraneous causes of variation" (Kogan, 1953) and as with analysis of variance, permits the significance of interaction factors to be determined.

There are drawbacks to controlled drug trials. As Hargreaves *et al.*, 1957 say, "Although blind controlled trials are essential, they are exceedingly time-consuming." This can be overcome by using small numbers of patients. A method of sequential analysis (Armitage, 1950, 1957; Bross, 1952) has been described which is compatible with small sample sizes, and a controlled clinical trial using this method is described by Snell and Armitage (1957). Where one is attempting to evaluate the effect of a drug on a number of symptoms (e.g. anxiety, tension) and physical attributes (e.g. weight, appetite) it would be possible (Armitage, 1954) "to set up a sequential scheme for each characteristic separately, and stop the trial when a conclusion could be safely reached about some defined combination of measurements". The technique allows for individual preferences of patients for drugs and might overcome the valid objection voiced by Davies and Shepherd (1955) in that "Improvement was among the drug-treated patients as a group, and no information was obtained about the response to be expected in particular patients." Rushbrooke *et al.*, 1956 describe a trial with small samples, the drugs' efficacy being ranked by the patients themselves. There are drawbacks with psychiatric patients of relying exclusively on their opinion. and Snell and Armitage found as an additional difficulty that patients were rarely able to give a clear set of preferences for a particular drug. Foltz *et al.*, 1955 describe a trial using bio-assay statistics. Drug responses were given arbitrary ratings, the drugs being prescribed at various dose levels. The percentage of total possible drug responses were plotted against the logarithm of drug doses. (Gaddum, 1933, discussed the advantages of plotting doses on a logarithmic scale.) Equivalent potencies of the drugs could then be determined.

Assessment of Drug Effect. Assessment of drug effect may be subjective (by testee and/or observer) or objective (tests performed by patient). The ideal would be to combine as many subjective and objective criteria as expedient. Foltz *et al.* (1955) prefer to rely on the testee's opinion, averring that "objective tests require either mental activity or physical participation by the testee, which may modify or interfere with the drug's hypnotic effect". Assessment by the observer alone may also be erroneous. Mitchell (1956) suggested that evaluation of improvement by individual interview lacks objectivity, while Elkes and Elkes (1954) consider that a "false picture may be conveyed if undue reliance is placed on clinical interview alone". The nurse's impression was relied on in drug trials by Lasagna (1954) and Straus *et al.*, 1955, the latter

regarding it as consistently superior to that of the patient. Quantification of drug response by arbitrary rating rather than clinical assessment may be helpful, although Lorr (1954) comments, "attempts to refine clinical judgment with rating-scales and check-lists have not yet proved the superiority of such measures".

A statistically sophisticated paper concerning a trial of five tranquillizing drugs in psychoneurosis came from Raymond *et al.*, 1957. The patient "was asked to record his judgment of the effect of each drug day by day" on a questionnaire. "No objective rating by the interviewing psychiatrist was attempted." The authors found that four of the drugs were no better than placebo, although amylobarbitone was. However, Glaser (1953) and Glaser and Whittow (1953, 1954) have shown that completing questionnaires may give apparent responses with no drug, and repeated completion of questionnaires may diminish the number of responses, giving a false impression of habituation to the drug. Moreover, Imboden and Lasagna (1956) found a tendency for psychiatric patients to underestimate drug effects as compared with assessments by nurse observers. Findings such as these may explain why two of the above authors (Raymond and Lucas, 1956) from reports of psychoneurotics at clinical interview, had previously concluded that patients with anxiety and tension respond favourably to benactyzine as compared with placebo.

It might seem that too great an emphasis has been laid upon methodology and correct appraisal of drug response. However, "the need for statistical methods in therapeutic trials arises largely from the variability in response from one individual to another" (Robson and Keele, 1951) and it may be that variability is greater in psychiatric patients than normals. Certainly the standard deviations for results from schizophrenics are greater than those from normals (Hoskins, 1946).

PHARMACOLOGICAL MODELS

Having considered the factors contributing to, and symptomatic variability of, response to drugs—together with the statistical methods for validating response, some pharmacological models having special reference to psychiatry will be discussed. The range is considerable and only salient aspects can be dealt with embracing work on both animals and humans. It is disconcerting to find Macht even in 1920 suggesting apropos animal work that "the field of what may be termed psycho-pharmacology is virgin soil, full of possibilities".

Analogies drawn from animal work, particularly in the behavioural field, are likely to be unrealistic. Miller (1957a) indicates that "behavioural studies do not yield completely pure measures" and that some of the screening tests "may be measuring only side-effects that are reasonably specific, but irrelevant to the clinically useful effects of the drug" (Miller, 1957b). Apart from variability within species, there is variability between species—witness the difference of LD 50s for LSD and ergonovine between mice, rats and rabbits (Cerletti, 1956). Similarly the LD 50 for cerebral depressants is greater in mice and rats than higher animals. This should make us chary of transferring drug data from animal to animal let alone from animal to man. Laurence and Pond (1958) ascribed the relative failure of the tranquillizers to achieve that claimed for them, to the fact that "new drugs are perforce developed in the first place by animal experiments, that . . . are at present irrelevant to the clinical use to which tranquillizers are put".

1. *Drugs and Normal Behaviour.* As early as 1898 investigators were interested in the effect of alcohol on the rat's activity (Stewart, 1898). A prod-

gious number of stimulant and depressant substances have been subsequently tested as to their effect on activity (Shirley, 1929; Searle and Brown, 1957). Refined techniques for quantifying animal movements have been developed such as the jiggle-cage of Tainter and co-workers (Schulte *et al.*, 1941). Interest was directed to the effect of drugs on learning and memory by use of maze experiments (McDowell and Vicari, 1921; Miller and Miles, 1936; Varner, 1933) and on the formation and extinction of conditioned reflex responses (Dworkin *et al.*, 1937; Gantt and Freile, 1944; Funderburk and Case, 1947). Dicker *et al.*, 1957 investigated the effect of methylpentynol on activity of rats using both an activity cage and a cruciform-shaped runway. The effect of the drug was to augment general activity and increase exploratory behaviour of the rat in the runway—in contrast to the effect of an equi-molecular dose of ethanol which decreased exploratory behaviour. Almost analogous investigations are made in man and Hilgard (1948) comments, "The pharmacologist uses animal subjects in the try-out stages, to the extent that he finds that animals react somewhat comparable to man. He rests finally, however, only when he has established his findings on man."

Earlier reviews of the psychological effects of drugs were by Poffenberger (1914, 1916, 1917, 1919), Meyer (1922), Darrow (1929), Spragg (1941), Gray and Trowbridge (1942). This work has been criticized by Eysenck (1957c) as not forming part of a theoretical system and not leading to any rational prediction. Similar censure was passed by Trouton (1958). Eysenck postulates that depressant drugs increase central inhibition whereas stimulant drugs have the opposite effect. He gives references supporting his theory that stimulants should decrease reaction time, improve performance on psychomotor and intellectual tasks, increase tapping rate, inhibit ergographic fatigue, while depressant drugs have the opposite effect (Eysenck, 1957d). Other work from this laboratory concerns the effect of drugs on the after-effects of the Archimedes spiral and work decrement (Eysenck *et al.*, 1957a, b). The prediction (from his theory) that stimulant drugs would prolong after-effect and delay work decrement, came true.

It was shown too that the ingestion of amylobarbitone sodium is associated with an increase of extraversion as measured on the Guilford R scale (Franks and Laverty, 1955; Laverty, 1958). Franks and Laverty demonstrated that the drug depresses the formation of conditioned eyeblink responses, whereas (Franks and Trouton, 1958) amphetamine facilitates their formation. Work such as Eysenck's accepts for its credo that variable response to drugs may be in part and even a major part, determined by personality. Such a theory, to be comprehensive, must be able to explain phenomena such as drug specificity, tolerance and susceptibility. The ideas of McDougall and Sheldon regarding susceptibility have already been mentioned, including those of Shagass which were accepted by Eysenck. Shagass's work has come in for criticism and it is almost certain that susceptibility is not simply related to the extraversion-introversion continuum of personality.

If the work of psychologists would seem to lack pharmacological sophistication, they have nevertheless pointed the need for objectification of drug response. This has been sporadically applied by clinical workers. Objective criteria were used by Osmond (1956) and Abramson (1956) when investigating the effect of LSD. Abramson tackled the problem of tolerance and found he could predict acquisition and loss of tolerance to LSD from questionnaire responses. Loss of tolerance could be represented by the formula $\log \frac{A}{A-x} = k_1 t$, where A is initial tolerance to LSD, x the amount of tolerance lost in time t,

and k_4 a rate constant. Idestrom (1954) demonstrated tolerance to phenobarbitone on flicker fusion. Hoffer (1957) employed objective tests in assessing response to adrenolutin. Dicker and Steinberg (1957) found 0.5 g. methylpentynol depressed autonomic reactions to a difficult motor task, and impaired the level of aspiration for performance as well as performance, results in contradiction to those of Trotter (1954) and Galley and Trotter (1958).

No unifying theme emerges from such work, apart from that inherent in the pharmacological action of the drug. Eysenck's attempts to predict effects in terms of personality must therefore warrant interest even if his conclusions be premature. It may be significant that Brengelmann (1958) found that "the results obtained with amytal and amphetamine are better understood on the basis of the implied pharmacological than from personality theory (Eysenck's) point of view".

One fruitful development in this field has been a better understanding of the placebo responder. Jellinek (1946) who investigated the comparative effectiveness of analgesics and found "an example of the rare U-shaped distribution" in his population, concluded there were individuals who tend to respond and individuals who do not tend to respond to placebos. Similar conclusions were reached by Beecher *et al.*, 1953 and Lasagna *et al.*, 1954. Trouton (1957) suggested that secondary rather than primary suggestibility was the trait related to placebo reactions, a trait not associated with any known personality dimensions.

If the field of psychopharmacology is to prosper, even in the absence of an integrating motif, sound pharmacological tenets must be adopted. The policy of determining drug effects at a single dose level should be recognized as fallible and proper dose-response curves constructed. This would lessen the possibility of recording artefacts of drug action as significant which could be shown to fall outside the dose-response range. A case in point is that of methylpentynol. A daily maximum dose level was initially recommended which was later discovered to fall in the toxic and not the therapeutic dose range (Marley and Bartholomew, 1958).

Of more potential interest to the psychiatrist is the relation of drugs to abnormal behaviour.

2. *Drugs and Abnormal Behaviour.* Pavlov (1927) described a method for producing "experimental neurosis". Considerable objections have arisen to this term, and it may be happier to substitute that suggested by Russell (1951) of "aberrant behaviour". Pavlov found that dogs developed behaviour resembling neurotic disturbance in man. Such disorganization of behaviour occurs when "incompatible response tendencies of similar strengths are simultaneously elicited under experimental conditions" (Russell, 1953a). Pavlov found that bromides ameliorated these disturbances in certain types of dogs. Not unexpectedly there is a species difference, Dworkin (quoted by Gantt, 1944) noting that hyperactive cats do not respond favourably to bromides. Developments along similar lines were made by Masserman (1943) and Maier (1949) ultimately inspiring work such as that of Jacobsen and Skaarup (1955a, b) who studied the modification of conflict behaviour in cats by anticholinergic compounds.

To obtain a definite answer in such experiments "the experimenter has invariably to restrict the animal's normal ways of behaving" (Katz, 1953). This is dwelt on by Russell (1953b). "Although such conflicts appear to be essential to the development of behaviour disorders they alone are not completely adequate. They must be accompanied by restraint of voluntary move-

ment, either physically, as in the case of the Pavlovian harness, or in terms of the subject's set, or past learning." Hebb (1947) who took for granted that the concept of neurosis is anthropomorphic as applied to animals, considered that the refusal of Masserman's cats to eat after feeding had been associated with a frightening air-blast was too specific to a particular situation to be identified with neurosis.

Brady (1957) feels that a more fruitful analysis of behaviour will stem from the operant conditioning techniques. Estes and Skinner (1941) first reported the technique of superimposing a conditioned emotional response on the lever pressing behaviour of rats. Since then the conditions for use of the free operant have been outlined by Skinner (1953) and Ferster (1953). As Brady and Hunt (1957) indicate, it is possible with this technique to study the effects of pharmacological agents "by separating the more specific emotional changes from the general behavioural and motor disturbances, debilitation, and the like that often appears as temporary and non-specific residuals of such treatments".

This may be a part answer to Chance (1957) who paraphrased the present situation thus: "The advent of the tranquilizers has found us completely unprepared. Some of the investigations throw up information of a non-specific nature. When the behaviour of the animal is used as the criterion of response, the attempt is not made to understand the behaviour but merely to define certain components which are then classified and modification by drugs noted". Chance indicates that a comprehensive notion of the normal behaviour of animals is required first as a yardstick for comparison.

Apart from modification of behavioural anomalies by drugs, aberrations of behaviour have been produced by drugs. De Jong found he could produce catatonic-like states in higher animals (1945a) with a wide range of substances (1945b). With inspired prescience (in view of the contemporary interest in indole, tryptamine and adrenaline derivatives) he examined a series of compounds related to mescaline and adrenaline for "catatonizing properties". Feldberg and Sherwood (1954, 1955) produced catatonic-like states in cats by intraventricular injections of dyflos (DFP), eserine, and bulbo-capnine, as did Schwartz *et al.*, 1956 with adrenochrome and adrenolutin. The behavioural changes were related only to the motor component of catatonia, and not to catatonic schizophrenia.

What conclusions then are to be drawn from the interaction of pharmacology and animal behaviour? Can the findings be translated in a modified form to man, or considered primarily as an essay in comparative pharmacology? Perhaps the most reasonable answer is that of Blough (1957) who deemed that "the most far-reaching value of behavioural research with drugs is that it may lead to a better understanding of basic laws governing the normal behaviour of individuals of all species".

Mescaline and LSD have been used to produce "model psychoses" in man. Denber (1957) insists that it is meaningless to speak of mescaline psychosis, as the response is unpredictable, not every patient developing the so-called psychosis. Fischer (1957) gives five reasons (which taken singly or together are not crucial) for assuming that the model psychosis is not a drug intoxication but related to schizophrenia. Hoffer (1956) concurs with this. Osmond and Smythies (1952) are more discriminating, comparing mescaline intoxication not with chronic, but with acute schizophrenia. Hoch (1956) is adamant that the "psychosis-producing agents and the blocking agents are non-specific in action". Rothlin and Cerletti (1956) also regard the LSD picture as devoid of specific features.

The discovery that LSD antagonized 5-hydroxy-tryptamine (5HT), a putative central transmitter, lent wider significance to the above findings. It was suggested that artificial psychoses and even schizophrenia might be due to inhibition or accumulation of 5HT in the brain. It is important to recognize that this work bears only an indirect relation to events in the central nervous system. For instance, LSD was first found to be a 5HT antagonist on muscle receptors *in vitro*, e.g. rat uterus, guinea-pig ileum (Gaddum and Hameed, 1954; Gaddum *et al.*, 1955; Savini, 1956; Woolley and Shaw, 1953) or *in vivo* (Salmoiraghi *et al.*, 1957). Gaddum and Picarelli (1957) concluded there were two kinds of tryptamine (5HT) receptor. LSD acts at the muscle or D receptors, but not at the M or ganglion receptors. To explain the effect of LSD, one may have to postulate both D and M receptors in the brain. That there may be a connection between 5HT activity at muscle receptors and central phenomena was suggested by Vane (1958) using the rat stomach strip (Vane, 1957). The hallucinatory potency of a number of drugs (amphetamine, mescaline) paralleled their activity on tryptamine receptors in the rat stomach. Moreover, tryptamine derivatives such as N,N dimethyl and diethyl tryptamine may produce model psychoses (Szára, 1957; Böszörményi and Brunecker, 1957) and even athetoid movements. The only parallel between the effect of 5HT on muscle receptors and possible central nervous system receptors is outlined by Woolley (1957). Apparently, rat or human oligodendroglia contract in the presence of 5HT, but not after the addition of 5HT anti-metabolites. Woolley suggests this is a possible way interference with brain 5HT leads to hallucinations and convulsions.

Data have appeared which make the relation between LSD and 5HT difficult to reconcile with a simple antagonism hypothesis. Thus 2-brom-LSD (BOL) is as potent a 5HT inhibitor as LSD *in vitro* and *in vivo* (Cerletti and Rothlin, 1955) but has no effect on the mental state in man (Snow *et al.*, 1955). Ginzel and Mayer-Gross (1956) demonstrated that pre-treatment with BOL would abolish the effects of LSD, whereas BOL given intravenously at the height of LSD symptoms had no effect. Bradley (1958) found synergism rather than antagonism between the central effects of 5HT and LSD in cats. Moreover, Lessin and Parkes (1957) suggest the antagonism of LSD and reserpine for 5HT is non-specific, while Gaddum and Vogt (1956) conclude that the central antagonism between 5HT and LSD is unrelated to peripheral antagonism between the two.

A possible link of such work with mental illness is that tranquillizing drugs which are alleged to alleviate schizophrenias also antagonize (or simulate) the effect of 5HT. Costa (1956) found that LSD and mescaline increase 5HT evoked contractions of the rat uterus, but that tranquillizing drugs antagonize the effect of 5HT. Gyermeck (1955) reported that chlorpromazine antagonizes the effect of 5HT *in vitro* and *in vivo*. Marrazzi (1957) demonstrated that cerebral synaptic inhibition produced by mescaline could be prevented by the tranquillizers. One clinical application is the use of iproniazid (which inhibits amine oxidase, a 5HT catabolite) for the treatment of depression (Costa *et al.*, 1957). Complete recovery in patients who would normally have only responded to electroplexy was found by Pare and Sandler (1958).

It is difficult to accept a simple one-to-one relation between 5HT and antagonizing substances for the production of predictable mental anomalies. To begin with, the central effects of a drug may not be reflected by their peripheral activity (as noted for 5HT, reserpine and LSD already by Gaddum and Vogt, 1956). Thus Meyers and Abreu (1952) compared quantitatively some synthetic atropine-like drugs both as to their effectiveness in producing central

phenomena and as peripheral acetylcholine blocking agents, and found no correlation between the two. Even drugs antagonizing 5HT may produce their effect by acting on other possible central transmitters. For example, chlorpromazine has atropine activity (Burn, 1954) and reserpine may deplete not only 5HT but also noradrenaline (Burn and Rand, 1957, 1958). To further complicate matters Elkes (1956) suggests that "rather than thinking in terms of acetylcholine, noradrenaline, and 5HT alone as possible neurohumoral mediators, it would be wiser to think in terms of families of compounds related to but not identical with the parent molecule". Vogt (1958) concludes that the "antagonistic effects of 5HT and LSD on behaviour depend on selective sensitization or inhibition of a characteristic group of centres by each drug and not on simple interaction by competition for the same receptors within the brain".

A more general thesis is presented by McIlwain (1957) who comments, "The relationship of chemotherapy to the central nervous system is inherent in the reaction of the body to chemical substances." He then goes on to quote Barcroft's proposition that "The fixity of the internal environment is in short the condition of mental activity" (Barcroft, 1934) implying that constancy in the composition of body fluids is more important to the functioning of the brain than it is to other body activities. This would account for central changes after substances which find difficulty in crossing the blood-brain-barrier, e.g. the hexamethonium compounds which may produce delirium (Smith, 1956) but because they are quaternary salts, central effects are precluded on account of permeability considerations (Paton, 1957).

These then are a few of the ramifications between psychiatry, response to drugs, and pharmacological investigation. Recently there has been a closer integration of these than hitherto. In conclusion, therefore, although one might like to agree with Tainter (1956) when he remarks with reference to experimental psychiatry, "the signs pointing to the right experimental approaches have been perceived, so that we may look forward to a period of unprecedented progress from what has been a most disheartening morass", one should remember as did Cholden (1956) that "Today psychiatry feels itself somehow to be at the crossroads. It may be the same crossroads that investigators have been many times in the past when important information seemed forthcoming." One should then temper enthusiasm with scepticism, and recall that Bertrand Russell defined scepticism as "not merely doubt, but what may be called dogmatic doubt".

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THE INADEQUATE PERSONALITY IN PSYCHIATRIC PRACTICE

By

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THIS paper is an attempt to clarify and give content to the concept of inadequacy of the personality, for the concept has gained considerable currency in psychiatry as a hypothesis to account for difficulties encountered in diagnosis, prognosis and treatment. Before the theory can be evaluated, it should be stated in a reasonably precise form. This enquiry starts from a very general statement about the idea of inadequacy as usually encountered in psychiatric practice, and proceeds towards a formulation in more exact terms.

The concept of inadequate personality is usually employed to denote some weakness of personality or character which accompanies or "underlies" the psychosis or neurosis, but is not identical with the latter, although influencing its course. Implicit in the concept is the idea of something long-lasting and difficult to change, operating in the previous personality, in the chain of events leading to breakdown, in the course of the breakdown itself and also subsequently. In this last phase inadequacy is suspected where there are unforeseen difficulties in resettlement, or a tendency to easy and frequent recurrence. In relation to treatment, inadequacy is often called in to account for unexpected failure of therapy, which, in prognostic terms, is equivalent to an unexpectedly bad outcome. In diagnosis it appears where the clinical picture is atypical.

This concept is clearly useful, but requires further clarification if it is to be more than an ingenious excuse for relative failure in diagnosis, prognosis or treatment. The ideal form of clarity would be a classification of the phenomena of inadequacy in aetiological terms, but in the present state of knowledge such an attempt is probably too ambitious, as the considerations which follow suggest.

Inadequacy is usually thought of as probably constitutional in origin, as arising, therefore, from a complex interaction of hereditary and early environmental influences. This view may well be correct, but it is of little help in the task of clarification. The authoritative opinion of Slater (1951) indicates that the hereditary element must almost certainly be of a multifactorial kind which could only be investigated by quantitative statistical analysis. This opinion is shared by Penrose (1951). Indeed, Slater's thesis suggests that a more precise definition of inadequacy in terms of traits is a necessary preliminary to probing its heredity. If this is so, it is putting the cart before the horse to try and clarify the concept of inadequacy by reference to its possible hereditary components.

It may seem more promising to link the concept of inadequacy with that of the character neuroses, thus throwing the emphasis on early environmental influences rather than on heredity. This view has much to commend it, especially as it commands a degree of general assent, which unfortunately breaks down when attempts are made to deal in precise terms. Reasonably exact formulations, such as Freud's "Anal-Erotic Character", tend to be accepted only by

followers of the school of the originator. This state of affairs applies not only to Freud, but to Mowrer's learning theory, concepts of conditioning, Meyer's Psychobiology and other theories of the development and organization of the personality. The enquirer in this sphere therefore finds himself faced by formulations of considerable precision, which only hold good within a relatively narrow frame of theoretical reference. These are of limited value to those unwilling to bind themselves to one particular school.

It is tempting to regard inadequacy and psychopathy as closely related. Judging by the frequency in clinical practice of such terms as "Hysterical Psychopath" and "Schizoid Psychopath", this is often done, but it is not always easy to know how precisely the terms are used. The issue may be to some extent linguistic, due to the use of the term "Inadequate" to describe one variety of psychopathy. The concept of inadequacy as described at the beginning of this paper, obviously has much in common with that of psychopathy, but without the particular emphasis on anti-social behaviour implied by the latter. This, however, does not take one a great deal further, as there is fairly general acceptance of Slater's (1951) view, that classifications of psychopathy so far evolved are more satisfying to their authors than to psychiatrists as a whole.

The work of behaviourist psychologists, exemplified among others by Cattell (1946), suggests the possibility of relating inadequacy and temperamental characteristics. This school has given considerable precision to the term "Temperament" and has indicated a number of factors which can justifiably be called aspects of temperament. This work, although sophisticated, is essentially descriptive. So far as the author is aware it has not been pursued far into the realms of aetiology.

Finally, there is the possibility of relating inadequacy to varieties of reaction to stress. At first sight this is an extremely promising approach, as the cardinal clinical feature of the inadequate personality is that he breaks down under stresses which the normal person survives, which, indeed, the doctor may have expected the inadequate himself to overcome. The Oxford Symposium on Stress (1958) is, however, distinctly discouraging. Sir Geoffrey Vickers was reported as follows: "The stressful situation which concerned the psychiatrist was produced not by events, but by the organism's interpretation of events in relation to itself. This in turn was a function of the way in which the individual personality was organized: and any conceptual model of this must include the organization of experience." Dr. Denis Hill did not think that the stressor could be defined: it owed its stressing quality to the nature of the individual, and it was the meaning of experience that mattered. These authoritative opinions suggest that before stress theory in psychiatry can become precise, solid progress must be made in solving some of the major problems of psychology. These include the organism's interpretative apparatus, a psychology of the self, learning theory and the organization of the personality, while the phrase about the meaning of experience faces the scientific enquirer with the rather dread presence of existentialism. Thus the approach to stress theory may be like the broad smooth road of the allegory, and lead to destruction.

There are good grounds, then, for regarding the aetiological approach as premature. Progress may be more possible on a descriptive-classificatory basis; that is, by returning to an earlier phase of the scientific method. Clinical history suggests that this approach may be sound, for the descriptive recognition of such syndromes as Addison's Disease preceded understanding of the morbid

processes involved, and indeed acted as a pointer to research workers as to where to apply their efforts. This paper tries to follow established clinical tradition in accepting descriptive precision as a pre-requisite to aetiological studies. The achievement of precision in present-day psychiatry, however, requires rather more emphasis on statistics than was necessary for the great clinical masters of the nineteenth century.

Considered statistically, the signs and symptoms of Addison's Disease (or Lobar Pneumonia or Bright's Disease) are a group of phenomena which show high positive intercorrelations, but low or negative correlations with other phenomena which may be common enough in medicine as a whole, but do not occur in Addison's Disease. These intercorrelated phenomena also co-vary; that is, they tend to appear and disappear together. Statistically, therefore, they form a correlation cluster. Addison's Disease is so striking a correlation cluster that it did not require statistical techniques to reveal its existence. Brilliant clinical observation was enough. Once it was described, however, it became a focal point for research which has led from the initial descriptive phase to greater aetiological understanding, and so to improved therapeutic control. In psychiatry the number of syndromes as striking as Addison's Disease is small. The correlation clusters which may prove to be fruitful areas for research cannot all be recognized by observation, but may require statistical techniques for their discovery. Once their existence is established, they have at first only descriptive, not aetiological status, but they can indicate where further research may transform descriptive recognition into aetiological understanding, and perhaps lead to therapeutic control.

This investigation studies certain correlation clusters yielded by the controlled observation of a mentally disordered population, and the possible relevance of some of these clusters to the problem of inadequacy. The details of the research yielding the clusters have been published elsewhere (Monro 1953, 1954). Briefly, a sample population of two hundred patients was chosen, to be as representative as possible of the disordered population of the country as a whole. Each patient was rated on a scale derived from that devised by Cattell (1946). This scale was so arranged as to give a comprehensive description of all aspects of the behaviour of the patients in the most economical terms. It consisted of 246 trait-terms and each patient was rated for the presence or absence of each trait. Operational definitions in terms of actual behaviour were worked out for each trait, as a guide to rating. Twenty of the patients were also rated by two colleagues, under test conditions, and positive correlations of the order of 0.85 were recorded between the three observers. Tetrachoric correlation coefficients were computed between the scores on each trait and every other trait. From these data, 35 groups of traits, or correlation clusters were derived. Each constituent trait of a cluster had a positive correlation of at least 0.55 with every other trait in the cluster. The full list of these clusters and their constituent traits is given in the works already referred to (Monro, 1953, 1954). At a later stage (Monro, 1956) the product-moment intercorrelations of the 35 clusters themselves were worked out in relation to the original sample population studied. These intercorrelations have some relevance to the present study.

These basic studies stemmed from the suggestion of Professor Drever of the Department of Psychology, University of Edinburgh, that it might be profitable to regard mental disorder as a defect or deviation of behaviour. This outlook is indeed fruitful, but requires discussion in relation to the study of inadequacy, for implicit in this viewpoint is the concept that all mentally dis-

ordered behaviour is inadequate. Socially speaking this is true, but inadequacy, in the sense used in this paper, is a term which excludes the recognized syndromes of mental illness and mental deficiency. It is also taken, in this study, to exclude personality types recognized as closely associated with particular forms of disorder, such as the cyclothyme temperament and the hysterical personality. It is the author's view that it should exclude the phenomena of psychopathy, so far as these can be shown to have a reasonably adequate descriptive basis. The data from the 35 clusters have some relevance to this latter point.

Consideration of the 35 clusters and their intercorrelations allows of their classification into broad groups:

1. Those corresponding to recognized psychiatric syndromes and morbid personality types: of these there are 14, including those relating to mental deficiency. Two clearly relate to psychopathy, one denoting the passive-inadequate psychopath, and the other the aggressive psychopath. It must be stated, however, that aggression is not really the keynote of this cluster, which describes essentially a predatory social attitude.

2. Those representing a capacity for mental health, or recovery potential: of these there are 5. Only those clusters have been included in this class which denote socially useful or desirable behaviour, and which also have no positive correlations of any magnitude with psychiatric syndromes.

3. Those representing modes of behaviour which may be socially desirable, and are certainly not socially undesirable, but which either have no positive correlations with class 2 above, or have positive correlations with psychiatric syndromes. Examples of the former kind are aesthetic interests and activities and ordinary sexual behaviour; examples of the latter are a rather strait-laced virtue which tends to be associated with obsessional states, and a rather self-denying humanitarian outlook which is often associated with depression. There are 6 of these clusters, which for the purpose of this paper may be regarded as "neutral".

The nature of inadequacy, as described in the first paragraph, suggests that if it appears at all, it should be shown by correlation clusters which do not represent psychiatric syndromes or either of the other two classes described above. Moreover the clusters denoting inadequacy should have relatively low positive correlations with a number of psychiatric syndromes, and negative correlations with the clusters denoting recovery potential. After eliminating the three classes of cluster described in the previous paragraph, ten remain, of which five fulfil these conditions adequately. These are described in detail below, in the order of the frequency of their occurrence in the sample population. The remaining five clusters are only dealt with briefly, as they are not directly relevant to a description of inadequacy, but have some importance as indicating behaviour patterns which fall somewhere between inadequacy and the other classes of cluster.

Cluster 1. The people denoted by this trait-group show failure to carry out tasks which they have been entrusted to complete, and which are within their powers. They make inadequate plans for fulfilling even obvious obligations, or, if they make them, they do not carry them out. Their sentiments do not remain attached for long to any one object, person or idea, and they are constantly changing their allegiances. Their social arrangements are free and easy, impromptu and left to chance. They tend to be off-hand in manner

and to ignore formalities and established punctilio. They are also lax in fulfilling social obligations.

Cluster 2. People in this trait-group have an exalted opinion of themselves, usually shown by talking a great deal about themselves and their affairs. They cannot tolerate unpalatable truths about themselves or their capacities, and take refuge from the risk of being shown up accurately by equivocation or flight into fantasy. They either refuse to believe propositions with strong personal significance for them, even when demonstrably true, or they believe such propositions in spite of strong evidence to suggest their falsity. They do not admit to being wrong in the absence of conclusive proof, and even then struggle to the last against the admission. They frequently blame others for the results of their own actions, and are more critical and fault-finding towards others than towards themselves. They have a strong tendency to grouse, and give the impression that they are the victims of hard luck, or they labour under a sense of injury or grievance.

Cluster 3. This trait-group denotes people who show few signs of pleasure or displeasure in human intercourse, whose predominant attitude to others is indifference. They do not necessarily feel inadequate in company, but prefer being on their own. They tend, however, to be stiff and stilted in manner, or clumsy and ungraceful in their movements, and are difficult to have dealings with. It is not easy to get them to make a forthright statement, as they seem to like mysteries and concealment. Nevertheless they show no eagerness to find out about things either at work, in the realm of ideas, or about their fellows.

Cluster 4. This group is composed of people who make no display of feeling in relationships in which strong feelings are usually shown. Many of them avoid intimate relationships and have few or none. They deprecate the display of emotion by others. They evade danger, difficulty, adversity and pain wherever possible, and complain excessively if evasion is impossible. They express a sense of grievance, of not having what is due to them, and often wish to change their position and circumstances although no good reasons exist for so doing.

Cluster 5. This trait-group denotes people who show ill-will to others on account of the latter's greater prosperity, endowment or achievement in any respect, associated with the sense of being outdone by those others. They look with a grudging eye on the success or good fortune of others, and desire to supplant them because they feel they have a right to the advantages enjoyed by those others. They think that others intend to hurt or belittle them when this is not the case, and often show this by unnecessary self-justification. They adhere firmly to their own opinions and avoid exposing them to critical investigation, and often display demonstrable prejudice.

These five behaviour patterns are ones which the clinician can recognize as familiar, in psychiatric wards, out-patient departments and even in the populace at large. They fulfil the statistical requirements for acceptance as descriptions of inadequacy. There are therefore grounds for accepting them as five types or categories of inadequate personality to be met with in psychiatric practice. There is, of course, no reason why an individual patient should not show signs of more than one of the above varieties of inadequacy.

DISCUSSION

The frequency of occurrence of these five clusters is given in the table below. Each patient in the sample population was, as described, rated for the presence or absence of the 246 traits in the comprehensive trait list. This rating was converted by the technique described elsewhere (Monro, 1953, 1954) into a score on a five point scale for each of the 35 clusters under consideration. The figure in the lower row of the table gives the percentage of the sample population with the maximum possible score on the cluster in the row above it.

		TABLE				
Cluster		1	2	3	4	5
Percentage	Frequency	27.5	27.0	21.0	10.0	7.5

The problem as to whether these behaviour patterns occur frequently in patients diagnosed as suffering from inadequacy presents certain difficulties. As long as the concept of inadequacy is somewhat vague, the diagnosis cannot be made with precision. A group of patients diagnosed as inadequates would therefore be relatively ill-defined, and its composition might vary greatly according to the outlook of the clinician selecting the sample. This paper has therefore approached the problem from the opposite direction. It has attempted to provide a more precise description of inadequacy. By further work it should be possible to determine whether this provides a tolerably satisfactory working hypothesis for dealing with the phenomena of inadequacy, as tentatively described in the first paragraph.

The five categories of inadequacy require some further consideration. Provisional titles might also save awkwardness in referring to them, but should not imply emphasis on any one aspect of the behaviour pattern at the expense of others.

The first cluster, in general terms, denotes lack of persistence or continuity of effort or attachment, while obligations are largely ignored. This is not simply lability, which appears quite separately associated with the surgent aspect of cyclothymia. Equally it is not anergia, which appears closely connected with depressive and schizophrenic states. It is also not mere frivolity of the "Butterfly" type, as it has no significant correlation with this quality. It does correlate fairly highly with the hysterical personality type, but not highly enough to warrant its inclusion as a hysterical manifestation. It may provisionally be called the Low Tenacity Type of Inadequacy.

The second cluster denotes hypersensitivity of the ego coupled with a rather passive kind of defensiveness, and may therefore be labelled Egoic Hypersensitivity.

The third cluster suggests a particular kind of social withdrawal which is nevertheless distinct from the emotional flattening common in schizophrenia. It may be provisionally entitled Diminished Social Responsiveness.

The fourth cluster, in contrast to the third, relates particularly to intimate relationships, and there is active deprecation of emotional display coupled with a sense of deprivation. This state of affairs can be called Feeling Avoidance.

The fifth cluster has a good deal in common with the second, and indeed has a fairly high positive correlation with it. Further investigation might show this group to have considerable egoic hypersensitivity, but it is more concerned with others than the second cluster. It is almost as though its members had discovered that attack is the best form of defence. It would appear to be related

to the "Othello" complex, and may provisionally be labelled Defensive Denigration.

The five remaining clusters may be briefly considered. Two of these show positive correlations only with depressive and obsessional states. One describes a tendency to excessive reserve, and the other indicates marked conventionality, meticulous conformity and a rather dependent outlook, especially in relation to authority. These may be either manifestations of depression or obsessional states, or aspects of the depressive or obsessional personality. In either case they are not at this stage relevant to this enquiry.

The other three clusters relate respectively to pedantic fussiness, bumptious pushfulness and a group which appears to be the exact opposite of this. They are rather simple, trustful and liable to be put upon. These are all patterns of behaviour which can lead to socially stressful situations, and may therefore be contributory to psychiatric illness. It may be that a full exploration of the concept of inadequacy will require to take more account of these patterns. For the present, however, they may be taken as lying in the hinterland between inadequacy and those behaviour patterns described above as "Neutral".

The data surveyed in this study therefore allow only of the description of five categories of inadequacy. It is suggested that this is a step in the direction of clarifying an obscure subject. If it provides starting points for further investigation of the subject, it will have fulfilled its purpose.

SUMMARY

The concept of "Inadequacy" or "Inadequate Personality" as this is usually used in psychiatric practice, is described. The difficulty of classifying inadequacy in aetiological terms is discussed, and an attempt to give greater clarity and precision to the concept, in descriptive terms, is described. This was done by applying statistical techniques to the data yielded by a previous study of a sample population of 200 patients. It was contended that behaviour patterns characteristic of inadequacy should be distinct from recognized psychiatric syndromes or from accepted types of pre-morbid personality. They should furthermore correlate negatively with behaviour patterns characteristic of recovery potential or tendencies to mental health. Finally they should have positive correlations of relatively low magnitude with a number of recognized psychiatric syndromes. Five behaviour patterns were found, which fulfilled these criteria, and their manifestations in terms of actual behaviour were described in detail. It was suggested that these patterns could be regarded as five types or categories of inadequacy, and they were given the following provisional titles Type 1: Low Tenacity; Type 2: Egoic Hypersensitivity; Type 3: Diminished Social Responsiveness; Type 4: Feeling Avoidance; Type 5: Defensive Denigration. The hope was expressed that this clarification of the concept of inadequacy might be a preliminary step towards a more precise investigation of the subject.

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POTENTIATION BY PHENOBARBITONE OF EFFECTS OF ETHYL ALCOHOL ON HUMAN BEHAVIOUR

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THERE is very little experimental evidence about the combined effects of alcohol and phenobarbitone, administered at or about the same time. Similarities in their actions suggest that the effects of the drugs are likely at least to summate in some respects (for example, in depressing the respiratory centre); and there are experimental and forensic observations which suggest that death has sometimes occurred after a dose of ethanol and phenobarbitone, neither of which would have been fatal by itself (Jetter and Maclean, 1943; Fisher, Walker and Plummer, 1948). Phenobarbitone and other barbiturates are widely prescribed (Dunlop, Henderson and Inch, 1952), usually without reference to the habitual alcohol consumption of the patient, and potentiation as well as summation has been reported to occur between some barbiturates and alcohol (Rachek, 1944; Sandberg, 1950).

It may be noted that much of the literature on the subject of potentiation *versus* summation lacks clarity in the definition of the terms. Very few of the reported observations on the effects of even alcohol alone are either well controlled or deal with accurate measurements of performance (Jellinek and McFarland, 1940). Interaction between drugs may produce effects upon behaviour (such as driving a car) which are of social and personal importance, and may even on occasions lead indirectly to the death of the subject. For all these reasons, further studies of the relationship between alcohol and phenobarbitone in the therapeutic dose-range are desirable. In the present work, observations have been made upon the time taken by normal healthy adult male subjects to react to visual and auditory stimuli, and upon their speed and accuracy in typing. Judgments by the subjects of their own performance have also been collected. A short account of the experiments has been given elsewhere (Joyce, 1958).

METHODS

The subjects were 8 male medical students (aged between 21 and 25 years) in their first or second clinical year. They were tested in groups of four at weekly intervals for four weeks. Each experimental session began approximately three

and a half hours after the subjects had last eaten (at lunch), and lasted about four and a half hours. During this time the subjects were kept together under general supervision; except when actually being tested they were allowed the run of a large laboratory (L_1) in which they were encouraged to feel comfortable and at ease, to read, write, play chess and talk about anything except the experiment or their "symptoms". They did not smoke. One experimenter (E_1) remained throughout in ignorance of the treatment each subject received. He recorded events in L_1 , conducted each subject to and from the experimental room (L_2) and issued subjects with their drugs (obtained from the second experimenter E_2 , who prepared them in a separate room (L_3) and had no social contact with the subjects at all). Two other experimenters operated the reaction-time apparatus, which was in L_3 , and recorded the results without seeing the subjects at any time during each experimental session. The general purpose of the experiment was outlined to all subjects at a special meeting (held some time before the first session) when they were also asked not to take alcohol on any experimental day, nor to take any barbiturate or other hypnotic for 24 hours beforehand.

The experimental design is shown in Table I. There were four treatments:

TABLE I
Design of the Experiment

Treatments	Drink				Capsules	
	Absolute Ethyl Alcohol (equivalent to)				Sodium Phenobarbitone	
	ml.				mg.	
A	..	..	..	..	60	0
B	..	..	..	..	30	65
C	..	..	..	..	0	130
D	..	..	..	..	0	0

Allocation of Treatments

Subject	..	..	1	2	3	4	5	6	7	8
Week										
1 (5)	..	..	A	B	C	D	A	B	C	D
2 (6)	..	..	B	A	D	C	C	D	A	B
3 (7)	..	..	C	D	A	B	D	C	B	A
4 (8)	..	..	D	C	B	A	B	A	D	C

Time Table of Treatments and Tests

Time: Minutes

0	..	..	..	..	Arrival
10	..	..	..	..	Test I—reaction time
50	..	..	..	..	Capsules
85	..	..	..	..	Drink
	..	..	..	..	Test II—reaction time
205	..	..	..	..	Dinner
215	..	..	..	..	Test III—reaction time
	..	..	..	..	Test IV—typing
	..	..	..	..	Departure

A (equivalent to 60 ml. of absolute ethyl alcohol), B (equivalent to 30 ml. of absolute ethyl alcohol+65 mg. phenobarbitone), C (130 mg. phenobarbitone) and D (control). The alcohol was administered in the form of an appropriate volume of gin, to which was added enough of a beverage (resembling cider but with an alcohol content of 1.25 per cent.) to fill a pint tankard. The phenobarbitone was given as the sodium salt in two gelatine capsules, each of which

contained either 65 mg. of the drug or a similar looking quantity of lactose. Thus every treatment involved the ingestion of two capsules and a pint of liquid. There are no grounds for thinking that the subjects could detect what was contained in the capsules which they took: the drinks fortified with gin were perceptibly different from those not fortified. Every subject received each of the four treatments once in the course of the four sessions: at each session one subject received each treatment. The sequence of treatments was allotted from a 4×4 Latin Square.

The apparatus used was that described by Bernstein (1950). One auditory and three visual stimuli were provided from a small box fixed on a bench in front of the seated subject, who was instructed to extinguish each stimulus by pressing the appropriate button with his preferred finger as rapidly as possible, and then to return his hand to a constant neutral position resting on the bench in front of the box. This was about 10×10 cm. in area, and distant about 35 cm. from the subject's eyes. The visual stimuli were provided by 6.3 V filament bulbs with blue, green and white filters, and the auditory stimulus by a buzzer. The buttons were connected to the stimuli in one of four predetermined orders, which had been judged by the five experimenters to be of equal difficulty in preliminary trials on themselves. The patterns of connections were allotted to subjects by a Latin Square arranged orthogonally to that for the treatment sequences. The same pattern was used for a given subject in the three trials of any one session.

Each test, of which there were three (I-III) in each session, consisted of 72 choice stimuli, of which 8 were dummies to lessen the very small probability of any subject perceiving the arrangement by rows of the remaining 64. The test was administered in the roughly constant time of 10 minutes.

On arrival, each subject was given a cake and a cup of tea in L_1 . He then took the first reaction-time trial (I). After being instructed by experimenter E_1 in his task, the subject was asked to predict (on all occasions except the first) how his performance would compare with his previous one. He was left alone during test I, and was asked by E_1 at the end to say how his performance had compared with his expectations. E_1 gave him two capsules, previously obtained from E_2 , with a little water and took him back to L_1 . Forty minutes later, E_1 gave the subject a tankard prepared by E_2 to contain the amount of alcohol appropriate to the subject's treatment. The subject always drank the contents within 10 minutes. Thirty-five minutes later, the subject took test II. When all subjects had completed II (i.e. at some point between 20 and 80 minutes after each individual had completed II), the whole group had dinner with E_1 at the students' hostel (five minutes' walk from the laboratory). After the return of the group, each subject was given test III, 120 minutes after his previous test. At the end of III he was asked to say, in addition to the usual judgment of performance, what treatment he thought he had received in the course of the evening. Finally he typed a copy of a passage from a paper by K. Z. Lorenz (1950) for five minutes (test IV). The passage was changed at each session and four passages were selected in advance for approximately equal levels of difficulty. The four subjects were tested successively, in the same order on a given day, so that the time intervals between testing and treatment were the same for all subjects on all occasions.

RESULTS

General

The subjects were at all times fully co-operative, although their behaviour, both as individuals and as groups, naturally differed considerably from one

occasion to another. For the most part, each subject was apparently content to remain in that part of the laboratory which had been allotted to him as his territory. Conversation was almost invariably about matters unconnected with the experiment, as had been requested: only once had a subject to be asked by E_1 to stop rehearsing his memory of the display pattern aloud, and reflecting on his own sensations—topics which were considered likely to put other subjects in possession of information about the experiment which might modify their responses unpredictably. Three subjects fell asleep at some point during a session (one after alcohol, one after both drugs together, and one after the dummy treatment): only one (the subject who had taken alcohol) needed to be gently but firmly woken.

Not every subject who had done so showed signs of having taken either drug, and indeed E_1 was unable to distinguish between those subjects who had taken phenobarbitone and those who had received only dummies. E_1 's guesses about who had taken alcohol (whether alone or with phenobarbitone), on the other hand, were correct on about 60–70 per cent. of the occasions. Such signs as were present (increased talkativeness, flushing, expansive movements) were most noticeable at the time of test II or at the start of dinner. They were usually absent by the time that subjects returned from dinner to the laboratory for the remaining tests.

Reaction Times

The mean complex reaction time for each trial of 64 stimuli is shown in Table II, together with the standard deviation for each trial. The values are

TABLE II

Complex Reaction Time: Mean (X) and Standard Deviation (Y) in Each Trial (64 Responses)

Subject		1		2		3		4	
		X	Y	X	Y	X	Y	X	Y
Week	Trial								
1	I ..	0.95	0.436	1.01	0.332	0.79	0.265	0.78	0.361
	II ..	0.80	0.300	0.86	0.173	0.66	0.200	0.65	0.173
	III ..	0.71	0.173	0.78	0.173	0.67	0.285	0.64	0.200
2	I ..	0.83	0.346	1.00	0.490	0.71	0.316	0.81	0.539
	II ..	0.63	0.224	0.90	0.224	0.70	0.285	0.58	0.332
	III ..	0.60	0.200	0.97	0.412	0.72	0.316	0.68	0.173
3	I ..	0.67	0.265	0.88	0.424	0.59	0.245	0.68	0.200
	II ..	0.63	0.265	0.80	0.245	0.81	0.400	0.73	0.141
	III ..	0.62	0.224	0.76	0.224	0.59	0.316	0.65	0.332
4	I ..	0.59	0.265	0.79	0.332	0.75	0.332	0.79	0.600
	II ..	0.50	0.141	0.85	0.316	0.87	0.400	0.63	0.173
	III ..	0.52	0.141	0.84	0.265	0.65	0.245	0.55	0.332
Subject		5		6		7		8	
		X	Y	X	Y	X	Y	X	Y
Week	Trial								
5	I ..	0.75	0.245	0.82	0.332	0.82	0.316	0.94	0.361
	II ..	0.64	0.200	0.71	0.141	0.75	0.141	0.77	0.245
	III ..	0.64	0.332	0.61	0.100	0.84	0.300	0.72	0.283
6	I ..	0.66	0.300	0.68	0.245	0.79	0.200	0.88	0.346
	II ..	0.62	0.200	0.76	0.200	0.73	0.200	0.84	0.300
	III ..	0.54	0.173	0.58	0.100	0.64	0.173	0.73	0.224
7	I ..	0.59	0.300	0.68	0.100	0.76	0.316	0.76	0.361
	II ..	0.55	0.224	0.62	0.300	0.71	0.173	0.66	0.200
	III ..	0.52	0.141	0.54	0.141	0.56	0.346	0.59	0.173
8	I ..	0.63	0.332	0.67	0.316	0.70	0.283	0.76	0.265
	II ..	0.59	0.265	0.67	0.224	0.63	0.224	0.79	0.447
	III ..	0.54	0.245	0.65	0.200	0.61	0.141	0.67	0.265

comparable with those usually obtained in this kind of test (Woodworth, 1938). The earlier reaction times in each trial were generally longer than were the later, which decreased towards a more or less steady value somewhat below the mean. There was considerable fluctuation in the times of successive responses; the information given by the time sequence of the observations was not used in the analysis, and the improvement with practice therefore contributes to the large standard deviation in each trial. The mean reaction time to visual stimuli was greater than that to auditory stimuli in every subject, but both responses followed a similar course and it does not appear useful to present the figures separately. It will be seen from Table II that the mean reaction time decreased in successive trials, and in successive weeks, and that any effects of drugs had to be detected against these progressive changes in performance. In fact, there is some disparity between the mean reaction times for different groups in the pre-treatment trials and this itself, though not significant ($p > 0.05$), indicates that the estimates are subject to considerable sampling variance.

TABLE III

Complex Reaction Time: Mean Reaction Time (X) and Mean Variability (Y) for Treatments and Trials
($\pm$ Standard Errors)

Treatment	I		II		III	
	Before Treatments		35 Minutes after Alcohol 75 Minutes after Phenobarbitone		2 Hours after Trial II	
	X	Y	X	Y	X	Y
Alcohol	0.79 ± 0.047	0.36 ± 0.049	0.73 ± 0.034	0.24* ± 0.026	0.67* ± 0.047	0.26 ± 0.034
Alcohol + Phenobarbitone	0.80 ± 0.042	0.32 ± 0.017	0.74 ± 0.037	0.23* ± 0.032	0.64† ± 0.030	0.23† ± 0.028
Phenobarbitone	0.75 ± 0.024	0.30 ± 0.043	0.69 ± 0.034	0.28 ± 0.034	0.68 ± 0.042	0.23 ± 0.021
Control	0.73 ± 0.044	0.32 ± 0.021	0.67 ± 0.038	0.22† ± 0.016	0.63* ± 0.033	0.19† ± 0.027

* Probability that difference from value I due to chance: $p < 0.05$ } one-tailed test
† Probability that difference from value I due to chance: $p < 0.01$ }

The smallest decrease (0.07 seconds) in mean reaction-time between trials I and III is shown by subjects treated with phenobarbitone. This difference is not significant ($p > 0.1$). A larger difference (0.10 second) ($0.05 > p > 0.02$) between the trials is shown by subjects on the dummy treatment; that shown by subjects treated with alcohol alone is larger again (0.12 second) ($0.05 > p > 0.02$). That of subjects treated with both drugs together is the greatest (0.16 second) ($p < 0.01$). The magnitude of the differences between trials I and II is the same (0.06 second) for all treatments, and is in no case significant: between trials II and III the only significant difference is shown by subjects treated with alcohol and phenobarbitone together.

The consistency of the response-times within each trial is indicated by the standard deviations which are also shown in Table III. The impression that they give to some extent resembles that given by the mean reaction-times themselves. The smallest reduction in variability (Δ s.d.=0.07 second) occurs with phenobarbitone but the greatest (Δ s.d.=0.13 second) only when dummies are taken.

The effects of alcohol, and of alcohol and phenobarbitone taken together, are intermediate and resemble each other (Δ s.d.=0.09–0.10 second).

It is curious and rather striking to note that the magnitude of the differences between the mean reaction times in the first and second trial is the same for all treatments (0.06 second), although the time of the second trial was chosen to occur when the effects of alcohol were expected to be, and appeared to be at their peak. In spite of behaving as if they were under the influence of alcohol, subjects so treated did not differ from the controls in the extent to which their reaction times were shortened at this trial. On the other hand, by the third trial there were striking differences in that the subjects treated with and apparently recovering from the effects of alcohol showed more reduction in reaction time than did the controls.

Typing

Only 2 of the 8 subjects normally used a typewriter to any extent. All subjects showed some improvement in performance with practice, but these effects were almost entirely confined to the first week. Table IV shows three measures obtained from the typing test. These are the speed, in letters per minute, the errors, as a percentage of the total number of letters typed; and the number of errors spontaneously corrected, as a percentage of the total number of errors made. The mean speed for all the subjects under all the treatments was 102 letters/minute (range 52–236), and the mean percentage of errors made was 11 per cent (range 6–30 per cent.). Neither showed any tendency to improve over successive weeks after the first. Subjects receiving the full dose of phenobarbitone typed more slowly than the other groups taken together ($0.05 > p > 0.02$). They made significantly fewer errors ($p < 0.01$) and corrected significantly more ($0.02 > p > 0.01$). Differences between the performance of subjects treated with alcohol alone and the control were small and except for number of errors made, not significant ($p > 0.2$); the tendency on this treatment was to type faster and make more errors. When alcohol and phenobarbitone were both taken, this tendency increased; although the dose of alcohol was smaller, subjects typed still faster and made still more mistakes.

TABLE IV
Measures of Typing Performance

Treatment	Speed Letters/Minute	Errors per 100 Letters	Errors Corrected per 100 Errors
Alcohol	104.8	12.0	26.5
Alcohol + Phenobarbitone ..	106.6	13.9	25.5
Phenobarbitone	96.6	7.6	40.5
Control	103.4	10.4	26.0
Standard error (obtained from analyses of variance)	± 4.10	± 1.51	± 2.61

Expectation and Judgment

Table V summarizes subjects' expectations before, and judgments after, performances on trials II and III. Subjects treated with alcohol, or with both drugs together, gave a greater number of definite statements (either in a positive or negative direction) than did those on the other two treatments. Those treated with phenobarbitone gave fewer definite statements than did the controls. All four groups tended to be pessimistic in anticipation of retrospect about trial II, but those treated with alcohol or with alcohol and phenobarbitone were

optimistic about III. The accuracy of subjects' expectations, or judgments, about the performances to which they related, showed no relationship to treatment. As performance usually improved between trials I and II and again between trials II and III the definite statements that were made about trial II were more often wrong, and those about trial III were more often right, no matter what the treatment. It is relevant to note that the subjects were medical students who had been taught, probably on a number of occasions, that alcohol impairs performance, and their general pessimism at trial II may owe something to this.

TABLE V
Expectation and Judgment

Treatment	II		III		Total "No Opinion"		
	Expected Better	Expected Worse	Judged Better	Judged Worse			
Alcohol	0	8	2	5	5	0	5
Alcohol + Phenobarbitone ..	2	6	0	8	4	0	4
Phenobarbitone	0	2	0	3	1	1	19
Control	1	2	1	5	2	1	15
	8		8		8		32

DISCUSSION

In these experiments there were several differences between the effects of phenobarbitone and those observed after ethyl alcohol. Subjects showed a smaller acceleration in performance and less reduction in variability in the complex reaction time test when they were treated with phenobarbitone than when they received only dummy treatments. After alcohol, on the other hand, they showed more acceleration and more reduction in variability. In the type-writing test, phenobarbitone reduced speed and increased accuracy, while after alcohol speed was increased very slightly and the number of errors was markedly increased. The number of definite predictions and judgments about performance was much increased by alcohol; it was if anything slightly reduced by phenobarbitone.

Since all these effects were observed three hours after the drugs were taken, when clinical signs of intoxication had disappeared and the concentration of alcohol in the blood was probably falling (Eggleton, 1941; Goldberg, 1951) they may have been due indirectly rather than directly to alcohol. Phenobarbitone is well known to persist in the circulation and presumably in the brain for a much longer period (Maynert and van Dyke, 1949) and the development of the effects over three or four hours occasions no surprise.

Whatever the explanation of the effects observed after alcohol, it is notable that larger effects in the same direction occurred when half the dose of alcohol was used in conjunction with phenobarbitone. It is possible though unlikely that the smaller dose of alcohol has greater effects than the larger dose: it is more probable that the responses were modified and augmented by the phenobarbitone. The sampling errors of the effects described are so great that not every individual mean is significantly different from the appropriate control value. However, when they are considered collectively (Table VI) the effect of phenobarbitone is seen to be in the opposite direction from that of alcohol in all cases except that of change in the variability of response. It might therefore have been expected that in combination the two drugs would be less active than either alone, especially as the dose of each when combined with the other was halved. But in every case except one (the reduction in the variability of the reaction-

time) the effect of a half dose of alcohol was greater in conjunction with phenobarbitone than that of a whole dose of alcohol alone. The probability that the effects summarized in Table VI would have arranged themselves by chance in the order shown is less than 0.01 (Friedman—2-way analysis of variance of ranked data (Siegel, 1956)).

TABLE VI
Summary of Changes in Performance Related to Treatments

Treatment	Reaction Times		Typing			Opinions Definite State- ments Per cent. Total
	Per cent. Decrease in Mean Trials I-III	Per cent. Decrease in Variability Trials I-III	Speed Letters/ Minute	Errors Per cent. Letters	Errors Un- corrected Per cent. Errors	
Phenobarbitone ..	9.3	23.3	96.6	7.6	59.5	41
Control ..	13.7	40.6	103.4	10.4	74.0	53
Alcohol ..	15.2	27.8	104.8	12.0	73.5	84
Alcohol+ Phenobarbitone	20.0	28.1	106.6	13.9	74.5	88

Although the measures obtained from the simpler and more complicated tasks are not exactly comparable (the former are measures of speed of response to stimuli that are not under the control of the subject, and the latter of rate of responding to stimuli which are under his control) it is apparent that the effects of a given treatment are similar in both cases. Had the number of errors made by subjects in the reaction-time trials also been recorded, it seems likely that they would have been found to vary in the same way as the errors made during typewriting. No comparison could be made between the extent to which errors made were recognized and corrected, because reaction-time errors were immediately evident to the subject: an incorrect response failed to extinguish the stimulus. However, although too much emphasis must not be placed on this at the moment, it is notable that changes of an ostensibly similar nature, due to the effect of drugs, occurred in both simple and rather more complex situations.

The complex reaction-time continued to diminish with practice, even under the influence of drugs, although simple reaction-time is usually reported to increase with alcohol (Jellinek and McFarland, 1940) or barbiturates (Goodnow, Beecher, Brazier, Mosteller and Tagiuri, 1951). The difference is probably in part due to the large part played by learning in the complex test. The practice effects, however, show a difference between the occasions when the subjects were treated with alcohol and when they were not, in that there was more improvement in performance after alcohol and phenobarbitone than when no drug or only phenobarbitone was administered. Also, the improvement occurred not when the blood alcohol concentration was presumably at its peak but some time later. The significance of this finding is obscure: it may represent compensation by the nervous system for the earlier effects of depression during the acute stage of intoxication. However, this is unlikely to provide a complete explanation, for other changes were also observed in the late phase, when the subjects appeared clinically to have recovered from the effect of alcohol; for instance, their typing was impaired, and the frequency with which they expressed definite opinions on their performance was increased. If these findings are confirmed they are of considerable practical significance in assessing how soon after exposure to alcohol a subject is likely to have regained his normal performances in various ways. It is commonly believed that subjects under the

influence of alcohol are more likely to have confidence in their own judgments, whether such confidence is justified or not. Comparison of the subjects' estimates at the time of trial III with their typing performance, both 3-4 hours after ingesting alcohol, show that at this time their confidence was still on the whole likely to be misplaced.

Results of the kind described can be interpreted in quite different ways according to the criteria that are employed. Thus, if speed or confidence of judgment are used as the criteria, it might be claimed that alcohol stimulates and phenobarbitone depresses performance: if accuracy in performing the tasks, or in assessing performance afterwards, are taken as the criteria, then the conclusions would have to be reversed. It is obviously never possible to measure all the aspects of performance which enter into even the simplest test situation, and it is therefore all the more important to specify those which have been studied. If this is done in sufficiently definite terms, many apparent contradictions in the experimental literature, such as those which concern the effects of even alcohol alone (Jellinek and McFarland, 1940) may prove to be unreal.

SUMMARY

1. The complex reaction times of 8 young healthy male subjects have been observed before and at two intervals after ingesting (the equivalent of) 60 ml. of ethyl alcohol, or 130 mg. phenobarbitone, or (the equivalent of) 30 ml. alcohol and 65 mg. phenobarbitone, or dummy treatments. Each subject received each treatment once, on separate occasions.

2. The mean reaction time decreased whether the subjects received drugs or not. Three hours after treatment, the decrease shown by those subjects who had received phenobarbitone was smaller, and by those who had received alcohol, or half doses of both drugs together, was greater than that shown by control subjects. The variability of reaction time also decreased, but all three treatments with drugs lessened the reduction of variability, in comparison with that shown by the control subjects.

3. Phenobarbitone slowed the rate at which subjects typed test passages, and under its influence they also made fewer errors and corrected more of the errors that they had made. With alcohol, subjects typed faster, and made more errors. When both drugs were taken together, the tendencies shown under alcohol were increased.

4. Subjects receiving alcohol or both drugs together made a larger number of definite statements about their past or future performances on reaction time tests. They also believed that they would perform, and had performed well at a time when their performance, as judged by typing errors, had significantly deteriorated.

5. Although the effects of phenobarbitone were more striking than, and usually in the opposite direction from those of alcohol, the effect of consuming half doses of both drugs together was in the same direction as and larger than that of taking a whole dose of alcohol alone. The systematic trends observed are very unlikely to have occurred by chance ($p < 0.01$). The results described support the belief that phenobarbitone potentiates some of the effects of ethyl alcohol.

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LEARNING THEORY AND BEHAVIOUR THERAPY*

By

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It would probably be true to say that the present position in the psychiatric treatment of neurotic disorders is characterized by the following features. (1) With the exception of electroshock, the only method of treatment at all widely used is psychotherapy. (2) In practically all its manifestations, psychotherapy is based on Freudian theories. (3) With the exception of intelligence testing, psychological contributions consist almost entirely in the administration and interpretation of projective tests, usually along psycho-analytic lines. I have argued in the past, and quoted numerous experiments in support of these arguments, that (1) there is little evidence for the practical efficacy of psychotherapy,† whether strictly Freudian or “eclectic” (8, 17); (2) that Freudian theories are outside the realm of science because of their failure to be consistent, or to generate testable deductions (10); and (3), that projective tests are so unreliable and lacking in validity that their use, except in research, cannot be defended (16).‡ I shall not here argue these points again; the evidence on which these views are based is quite strong, and is growing in strength every year. I shall instead try to make a somewhat more constructive contribution by discussing an alternative theory of neurosis, an alternative method of treatment, and an alternative way of using the knowledge and competence of psychologists in the attempted cure of neurotic disorders. It need hardly be emphasized that the brief time at my disposal will make it inevitable that what I have to say will sound much more dogmatic than I would like it to be; I have to ask your indulgence in this respect, and request you to bear in mind all the obvious qualifying clauses which, if included in this paper, would swell it to three times its present size.

Few psychiatrists are likely to deny that all behaviour ultimately rests on an inherited basis, but even fewer would be prepared to assert that environmental influences played no part in the genesis and modification of behaviour. Once we

* This paper was delivered on 3 July, 1958 to a meeting of the R.M.P.A., and its style inevitably bears traces of the fact that it was originally prepared for verbal presentation. It was followed by another paper, delivered by Mr. Gwynne Jones, giving concrete examples of the application of behaviour therapy from our own experience. Some of these are discussed in his article published in this Journal (29), and it is suggested that readers interested in the theories here advanced may like to consult this article in order to obtain some notion of the practical methods emanating from these theories. A more detailed discussion of many theoretical points that arise may be found in “Dynamics of Anxiety and Hysteria” (15), as well as several of my previous books (7, 9, 11).

† When I first suggested that the literature did not contain any kind of unequivocal proof of the efficacy of psychotherapeutic treatment, this conclusion was widely criticized. Since then, however, Dr. Weinstock, Chairman of the Fact-Finding Committee of the American Psychoanalytical Association, has explicitly stated in a lecture delivered at the Maudsley Hospital that his Association made *no claims of therapeutic usefulness for psychoanalytic methods*, and in this country Glover (21) has equally explicitly disavowed such claims. On this point, therefore, leading psychoanalysts appear to share my views to a considerable extent.

‡ This fact is also beginning to be more widely realized, and it is symptomatic that such well-known departments as that belonging to the New York Psychiatric Hospital have followed the lead of the Institute of Psychiatry and discontinued the routine use of projective techniques like the Rorschach.

are agreed that learning and conditioning are instrumental in determining the different kinds of reaction we may make to environmental stimulation, we will find it very difficult to deny that neurotic reactions, like all others, are *learned* reactions, and must obey the laws of learning. Thus, I would like to make my first claim by saying that modern learning theory (24), and the experimental studies of learning and conditioning carried out by psychologists in their laboratories (38) are extremely relevant to the problems raised by neurotic disorders (41). If the laws which have been formulated are, not necessarily true, but at least partially correct, then it must follow that we can make deductions from them to cover the type of behaviour represented by neurotic patients, construct a model which will duplicate the important and relevant features of the patient, and suggest new and possibly helpful methods of treatment along lines laid down by learning theory. Whether these methods are in fact an improvement over existing methods is, of course, an empirical problem; a few facts are available in this connection and will be mentioned later. It is unfortunate that insistence on empirical proof has not always accompanied the production of theories in the psychiatric field—much needless work, and many heart-breaking failures, could have been avoided if the simple medical practice of clinical trials with proper controls had always been followed in the consideration of such claims.

How, then, does modern learning theory look upon neurosis? In the first place, it would claim that neurotic symptoms are *learned patterns of behaviour* which for some reason or other are *unadaptive*. The paradigm of neurotic symptom formation would be Watson's famous experiment with little Albert, a nine months old boy who was fond of white rats (44). By a simple process of classical Pavlovian conditioning Watson created a phobia for white rats in this boy by standing behind him and making a very loud noise by banging an iron bar with a hammer whenever Albert reached for the animal. The animal was the conditioned stimulus in the experiment, the loud fear-producing noise was the unconditioned stimulus. As predicted, the unconditioned response (fear) became conditioned to the C.S. (the rat), and Albert developed a phobia for rats, and indeed for all furry animals. This latter feature of the conditioning process is of course familiar to all students as the generalization gradient (38); an animal or a person conditioned to one stimulus also responds, although less and less strongly, to other stimuli further and further removed from the original one along some continuum.

The fear of the rat thus conditioned is unadaptive (because white rats are not in fact dangerous) and hence is considered to be a neurotic symptom; a similarly conditioned fear of snakes would be regarded as adaptive, and hence not as neurotic. Yet the mechanism of acquisition is identical in both cases. This suggests that chance and environmental hazards are likely to play an important part in the acquisition of neurotic responses. If a rat happens to be present when the child hears a loud noise, a phobia results; when it is a snake that is present, a useful habit is built up!

The second claim which modern learning theory would make is this. People and animals differ in the speed and firmness with which conditioned responses are built up (39). Those in whom they are built up particularly quickly and strongly are more likely to develop phobias and other anxiety and fear reactions than are people who are relatively difficult to condition (15). Watson was lucky in his choice of subject; others have banged away with hammers on metal bars in an attempt to condition infants, but not always with the same success.

individual differences must be taken into account in considering the consequences of any course of attempted conditioning. Nor is the degree of conditionability the only kind of individual variability with which we are concerned. Learning theory tells us that the amount of reinforcement following any action determines in part the amount of conditioning that takes place (43). Thus the louder the noise, the greater the fright of the infant, and the greater the fright, the stronger the phobia. But different children have different types of autonomic system, and the same amount of noise produces quite unequal amounts of autonomic upheaval in different children. Consequently, autonomic reactivity must also be considered; the more labile or reactive the child, the more likely he is to produce strongly conditioned fear reactions, anxieties, and phobias. The individual differences in autonomic reactivity and in conditionability have been conceptualized as giving rise to two dimensions of personality, namely neuroticism and introversion respectively (11). The more autonomically reactive, the more prone will the individual be to neurotic disorders. The more easily he forms conditioned responses, the more introverted will his behaviour be. Combine introversion and neuroticism, and you get the dysthymic individual, the person almost predestined to suffer from anxieties, conditioned fears and phobias, compulsions and obsessions, reactive depressions and so forth.

But this is only part of the story. Many conditioned responses are unadaptive, and consequently may embarrass the individual and even drive him into a mental hospital if sufficiently intense. Yet other conditioned responses are obviously necessary and desirable; indeed, many of them are indispensable for survival. It has been argued very strongly that the whole process of socialization is built up on the principle of conditioning (35); the overt display of aggressive and sexual tendencies is severely punished in the child, thus producing conditioned fear and pain responses (anxiety) to situations in which the individual is likely to display such tendencies. He consequently refrains from acting in the forbidden manner, not because of some conscious calculus of hedonic pleasure which attempts to equate the immediate pleasure to be gained from indulgence with the remote probability of later punishment, but because only by not indulging, and by physically removing himself can he relieve the very painful conditioned anxiety responses to the whole situation. Anxiety thus acts as a mediating drive, a drive which may be exceedingly powerful by virtue of its combination of central, autonomic, skeletal, and hormonal reactions. This mediating role of anxiety, and its capacity to function as an acquired drive, have been subjected to many well conceived experimental studies, and the consensus of opinion appears to leave little doubt about the great value and predictive capacity of this conception (34).

Let us now consider an individual who is deficient in his capacity to form quick and strong conditioned responses. He will be all the less likely to be subject to phobias and other anxieties, but he will also be less likely to form useful conditioned responses, or to become a thoroughly socialized individual. When this lack of socialization is combined with strong autonomic drive reactions (high neuroticism), such an individual is likely to show the neurotic symptomatology of the psychopath or the hysteric, and indeed, in our experimental work we have found that, as predicted, dysthymic patients and normal introverts are characterized by the quick and strong formation of conditioned responses, while psychopaths and normal extraverts are characterized by the weak and slow formation of conditioned responses (12, 14, 15). Thus the deviation from the average in either direction may prove disastrous—too strong

conditioning easily leads to dysthymic reactions, too weak conditioning easily leads to psychopathic and hysterical reactions. The logic of this whole approach leads me to postulate two great classes of neurotic symptoms which between them exhaust in principle all the possible abnormal reactions with which you are all familiar. On the one hand we have *surplus conditioned reactions*, i.e. reactions acquired along the lines I have adumbrated, and where the reaction is unadaptive, even though originally it may have been well suited to circumstances. On the other hand we have *deficient conditioned reactions*, i.e. reactions normally acquired by most individuals in society, which are adaptive, but which because of defective conditioning powers have not been acquired by a particular person. It is necessary to emphasize that surplus conditioned reactions and deficient conditioned reactions are due to an interplay between such individual factors as conditionability and autonomic lability, on the one hand, and environmental conditions on the other. There will be no socialization for an individual who cannot form conditioned responses at all, but conversely, there will be no socialization for a person growing up on a desert island, however powerful his conditioning mechanism may happen to be. In this paper I have no time to deal with differences in the conditioning forces of the environment, and their relation to such factors as social class, but they should certainly not be forgotten.

Many other testable deductions, apart from the differential conditionability of dysthymics and hysterics, follow from such a formulation. Some of these deductions can be tested in the laboratory, and examples have been given in my book, *The Dynamics of Anxiety and Hysteria*. But others can be tested clinically, and for the sake of an example I shall give just one of these. I have shown how psychopathic reactions originate because of the inability of the psychopath, due to his low level of conditionability, to acquire the proper socialized responses. But this failure is not absolute; he conditions much less quickly and strongly than others, but he ~~does~~ conditions. Thus where the normal person may need 50 pairings of the conditioned and the unconditioned stimulus, and where the dysthymic may need 10, the psychopath may require 100. But presumably in due course the 100 pairings will be forthcoming, although probably much later in life than the 10 of the dysthymic, or the 50 of the normal person, and then he will finally achieve a reasonable level of socialization. If this chain of reasoning is correct, it would lead us to expect that the diagnosis "psychopath" would by and large be confined to relatively young people, say under thirty years of age; after thirty the course of life should have brought forth the required 100 pairings and thus produced the needed amount of socialization. As far as I can ascertain, clinical psychiatric opinion is in agreement with this prediction.

How does our theory compare with the psychoanalytic one? In the formation of neurotic symptoms, Freud emphasizes the traumatic nature of the events leading up to the neurosis, as well as their roots in early childhood. Learning theory can accommodate with equal ease traumatic single-trial learning, for which there is good experimental evidence (26), but it can also deal with repeated sub-traumatic pain and fear responses which build up the conditioned reaction rather more gradually (42). As regards the importance of childhood, the Freudian stress appears to be rather misplaced in allocating the origins of *all* neuroses to this period. It is possible that many neurotic symptoms find their origin in this period, but there is no reason at all to assume that neurotic symptoms cannot equally easily be generated at a later period provided conditions are arranged so as to favour their emergence.

The point, however, on which the theory here advocated breaks decisively with psychoanalytic thought of any description is in this. Freudian theory regards neurotic symptoms as adaptive mechanisms which are evidence of repression; they are "the visible upshot of unconscious causes" (37). Learning theory does not postulate any such "unconscious causes", but regards neurotic symptoms as simple learned habits; there is no neurosis underlying the symptom, but merely the symptom itself. *Get rid of the symptom and you have eliminated the neurosis.* This notion of purely symptomatic treatment is so alien to psychoanalysis that it may be considered the crucial part of the theory here proposed. I would like to explore its implications a little further later on.

From the point of view of learning theory, treatment is in essence a very simple process. In the case of surplus conditioned responses, treatment should consist in the extinction of these responses; in the case of deficient conditioned responses, treatment should consist in the building up of the missing stimulus-response connections. Yet this apparent simplicity should not mislead us into thinking that the treatment of neurotic disorders offers no further problems. It is often found in scientific research that the solution of the problems posed by applied science is as complex and difficult as is the solution of the problems posed by pure science; even after Faraday and Maxwell had successfully laid the foundations of modern theories of electricity it needed fifty years and the genius of Edison to make possible the actual application of these advances to the solution of practical problems. Similarly here; a solution in principle, even correct, still needs much concentrated and high-powered research in the field of application before it can be used practically in the fields of cure, amelioration, and prophylaxis.

What are the methods of cure suggested by learning theory? I shall give two brief examples only, to illustrate certain principles; others have been given by G. Jones (29). One method of extinguishing the neurotic response X to a given stimulus S is to condition another response R to S, provided that R and X are mutually incompatible. This method, called "reciprocal inhibition" by Wolpe (45, 46), harks back to Sherrington (40) of course, and may be illustrated by returning to our rat phobic little boy. Essentially what Watson had done was to condition a strong sympathetic reaction to the sight of the rat. If we could now succeed in establishing a strong parasympathetic reaction to the sight of the animal, this might succeed in overcoming and eliminating the sympathetic response. The practical difficulty arises that, to begin with at least, the already established conditioned response is of necessity stronger than the to-be-conditioned parasympathetic response. To overcome this difficulty, we make use of the concept of stimulus gradient already mentioned. The rat close by produces a strong conditioned fear reaction; the rat far away in the distance produces a much weaker reaction. If we now feed the infant chocolate while the rat is being introduced in the far distance the strong parasympathetic response produced by the chocolate-munching extinguishes the weak sympathetic response produced by the rat. As the conditioned parasympathetic response grows in strength, so we can bring the rat nearer and nearer, until finally even close proximity does not produce sympathetic reactions. The sympathetic reaction has been extinguished; the phobia has been cured. This is in fact the method which was used experimentally to get rid of the experimentally induced fear (27), and it has been used successfully by several workers in the field of child psychiatry. More recently Herzberg (23) has developed his system of active psychotherapy, and more particularly, Wolpe (46) in his

psychotherapy by reciprocal inhibition, have shown that these principles can be applied with equal success to the severe neuroses of adult men and women—substituting other methods, of course, for the chocolate-munching, which is more effective with children than with adults!

As an example of the cure of deficient conditioned responses, let me merely mention *enuresis nocturna*, where clearly the usual conditioned response of waking to the conditioned stimulus of bladder extension has not been properly built up. A simple course of training, in which a bell rings loudly whenever the child begins to urinate, thus activating an electric circuit embedded in his bedclothes, soon establishes the previously missing connection, and the extremely impressive list of successes achieved with this method, as compared with the very modest success of psychotherapeutic methods, speaks strongly for the correctness of the theoretical point of view which gave rise to this conception (36).

We thus have here, I would suggest, an alternative theory to the Freudian, a theory which claims to account for the facts at least as satisfactorily as does psychoanalysis, and which in addition puts forward quite specific suggestions about methods of treatment. I have called these methods "behaviour therapy" to contrast them with methods of psychotherapy.* This contrast of terms is meant to indicate two things. According to psychoanalytic doctrine, there is a psychological complex, situated in the unconscious mind, underlying all the manifest symptoms of neurotic disorder. Hence the necessity of therapy for the psyche. According to learning theory, we are dealing with unadaptive behaviour conditioned to certain classes of stimuli; no reference is made to any underlying disorders or complexes in the psyche. Following on this analysis, it is not surprising that psychoanalysts show a preoccupation with psychological methods involving mainly *speech*, while behaviour therapy concentrates on actual *behaviour* as most likely to lead to the extinction of the unadaptive conditioned responses. The two terms express rather concisely the opposing viewpoints of the two schools. Table I presents, in summary form, a tabulation of the most important differences between Freudian psychotherapy and behaviour therapy.

What kind of answer would we expect from the Freudians? I think their main points would be these. They would claim, in the first place, that conditioning therapy has frequently been tried, but with very poor results; aversion therapies of alcoholism are often mentioned in this connection. They would go on to say that even where symptomatic treatments of this kind are apparently successful, as in enuresis, the symptom is likely to return, or be supplanted by some other symptom, or by an increase in anxiety. And, in the third place, they

* The growth of the theoretical concepts and practical methods of treatment subsumed in the term "behaviour therapy" owes much to a large number of people. Apart from Pavlov and Hull, who originated the main tenets of modern learning theory, most credit is probably due to Watson, who was among the first to see the usefulness of the conditioned paradigm for the explanation of neurotic disorders; to Miller and Mowrer, who have done so much to bring together learning theory and abnormal human behaviour; to Spence, whose important contributions include the detailed analysis of the relation between anxiety and learning; and to Wolpe, who was the first to apply explicitly some of the laws of learning theory to the large scale treatment of severe neurotics. If there is any novelty in my own treatment of these issues it lies primarily: (1) in the pulling together of numerous original contributions into a general theory and (2) in the introduction into this system of the concepts of neuroticism and extraversion/introversion as essential parameters in the description and prediction of behaviour. I would like to emphasize, however, that this contribution could not have been made had the ground work not been well and truly laid by the writers quoted above and by many more, only some of whom are quoted in the bibliography.

TABLE I

Freudian Psychotherapy

- 1. Based on inconsistent theory never properly formulated in postulate form.
- 2. Derived from clinical observations made without necessary control observation or experiments.
- 3. Considers symptoms the visible upshot of unconscious causes ("complexes").
- 4. Regards symptoms as evidence of *repression*.
- 5. Believes that symptomatology is determined by defence mechanism.
- 6. All treatment of neurotic disorders must be *historically* based.
- 7. Cures are achieved by handling the underlying (unconscious) dynamics, not by treating the symptom itself.
- 8. Interpretation of symptoms, dreams, acts, etc. is an important element of treatment.
- 9. Symptomatic treatment leads to the elaboration of new symptoms.
- 10. Transference relations are essential for cures of neurotic disorders.

Behaviour Therapy

- 1. Based on consistent, properly formulated theory leading to testable deductions.
- 2. Derived from experimental studies specifically designed to test basic theory and deductions made therefrom.
- 3. Considers symptoms as unadaptive conditioned responses.
- 4. Regards symptoms as evidence of faulty learning.
- 5. Believes that symptomatology is determined by individual differences in conditionability and autonomic lability, as well as accidental environmental circumstances.
- 6. All treatment of neurotic disorders is concerned with habits existing at *present*; their historical development is largely irrelevant.
- 7. Cures are achieved by treating the symptom itself, i.e. by extinguishing unadaptive C.R.s. and establishing desirable C.R.s.
- 8. Interpretation, even if not completely subjective and erroneous, is irrelevant.
- 9. Symptomatic treatment leads to permanent recovery provided autonomic as well as skeletal surplus C.R.s are extinguished.
- 10. Personal relations are not essential for cures of neurotic disorder, although they may be useful in certain circumstances.

ould claim that even if in some cases the therapies suggested might be successful, yet in the great majority of cases psychoanalysis would be the only method to produce lasting cures. Let me deal with these points one by one.

There is no doubt that conditioning treatment of alcoholism has often been tried, and that it has often failed. I have no wish to take refuge in a *quoque* argument by pointing out that alcoholism has been particularly difficult to treat by any method whatever, and that psychoanalytic methods also have been largely unsuccessful. I would rather point out that learning theory is an exact science, which has elaborated quite definite rules about the establishment of conditioned reflexes; it is only when these rules are properly applied by psychologists with knowledge and experience in this field that the question of success or failure arises. Thus it is quite elementary knowledge that the conditioned stimulus must precede the unconditioned stimulus if conditioning is to take place; backward conditioning, if it occurs at all, is at best very weak. Let some workers in the field of alcoholism have used a method in which the unconditioned stimulus regularly preceded the conditioned stimulus; under these conditions learning theory would in fact predict the complete failure of the experiment actually reported! Again, the time relation between the applica-

tion of the conditioned stimulus and the unconditioned stimulus is a very important one; it is controlled to very fine limits of hundredths of a second in psychological experimentation, and it has been universally reported that conditioning in which any but the optimal time relation is chosen is relatively ineffective. Taking eye-blink conditioning as an example, it is found that a time interval of about $\frac{1}{2}$ second is optimal, and that with intervals of $2\frac{1}{2}$ seconds no conditioning at all takes place (31, 32). No attention seems to have been paid to these points by most workers on alcoholism, who apply the conditioned and unconditioned stimuli in such a vague way that it is often impossible to find out what the actual time relations were. This lack of rigour makes it quite impossible to adduce these so-called experiments as evidence either in favour or against conditioning therapy (19).

How about the return of symptoms? I have made a thorough search of the literature dealing with behaviour therapy with this particular point in view. Many psycho-analytically trained therapists using these methods have been specially on the lookout for the return of symptoms, or the emergence of alternative ones; yet neither they nor any of the other practitioners have found anything of this kind to happen except in the most rare and unusual cases (35). Enuresis, once cured by conditioning therapy, remains cured as a general rule; relapses occur, as indeed one would expect in terms of learning theory under certain circumstances, but they quickly yield to repeat treatment. So certain of success are the commercial operators of this method that they work on a "money back if unsuccessful" policy; their financial solvency is an adequate answer to the psychoanalytic claim. Nor would it be true that alternative symptoms emerge; quite the contrary happens. The disappearance of the very annoying symptom promotes peace in the home, allays anxieties, and leads to an all-round improvement in character and behaviour. Similar results are reported in the case of major applications of behaviour therapy to adults suffering from severe neurotic disorders; abolition of the symptom does not leave behind some mysterious complex seeking outlet in alternative symptoms (35). Once the symptom is removed, the patient is cured; when there are multiple symptoms, as there usually are, removal of one symptom facilitates removal of the others, and removal of all the symptoms complete the cure (46).

There is one apparent exception to this rule which should be carefully noted because it may be responsible for some of the beliefs so widely held. Surplus conditioned reactions may themselves be divided into two kinds, autonomic and motor. Anxiety reactions are typical of the autonomic type of surplus conditioned reactions, whereas tics, compulsive movements, etc., are typical of motor conditioned reactions. What has been said about the complete disappearance of the symptom producing a complete disappearance of the neurosis is true only as far as the autonomic conditioned reactions are concerned. Motor reactions are frequently activated by their drive-reducing properties *vis-à-vis* the historically earlier conditioned autonomic responses (35); the extinction of the motor response without the simultaneous extinction of the conditioned autonomic response would only be a very partial cure and could not be recommended as being sufficient. As pointed out at the end of the previous paragraph, "removal of *all* the symptoms completes the cure", and clearly removal of the motor conditioned response by itself, without the removal of the autonomic conditioned response is only a very partial kind of treatment. Behaviour therapy requires the extinction of all non-adaptive conditioned responses complained of by the patient, or causally related to these symptoms.

But how frequently does this type of treatment result in cures? Again I have made a thorough search of the literature, with the following outcome. G. P. treatment, not making use of psychotherapy in any of its usual forms, results in a recovery of about two seriously ill neurotics out of three (4). Eclectic psychotherapy results in a recovery of about two seriously ill neurotics out of three (8). Psychotherapy by means of psychoanalysis fares slightly worse, but results are at a comparable level (17). Results of behaviour therapy of seriously ill neurotics, as reported by Wolpe, are distinctly superior to this, over 90 per cent. recovering (46). This difference is highly significant statistically, and it should be borne in mind that the number of sessions required by behaviour therapy is distinctly smaller than that required by psychotherapy, whether eclectic or psychoanalytic. (Wolpe reports an average of about 30 sittings for his cases.)

These results are encouraging, but of course, they must not be taken too seriously. Actuarial comparisons of this kind suffer severely from the difficulty of equating the seriousness of disorders treated by different practitioners, the equally obvious difficulty of arriving at an agreed scale for the measurement of "recovery", and the impossibility of excluding the myriad chance factors which may effect gross behaviour changes of the kind we are here considering. I would not like to be understood as saying that behaviour therapy has been *proved* superior to psychotherapy; nothing could be further from my intention. What I am claiming is simply that as far as they go—which is not very far—available data do not support in any sense the Freudian belief that behaviour therapy is doomed to failure, and that only psychoanalysis or some kindred type of treatment is adequate to relieve neurotic disorders. This Freudian belief is precisely this—a belief; it has no empirical or rational foundation. I have no wish to set up a counter-belief, equally unsupported, to the effect that psychotherapy is doomed to failure, and that only behaviour therapy is adequate to relieve neurotic disorders. What I would like to suggest is simply that a good case can be made out, both on the theoretical and the empirical level, for the proposition that behaviour therapy is an effective, relatively quick, and probably lasting method of cure of some neurotic disorders. This case is so strong that clinical trials would appear to be in order now to establish the relative value of this method as compared with other available methods, such as psychoanalysis, or electroshock treatment. Even more important, I think the evidence would justify psychiatrists in experimenting with the method, or rather set of methods, involved, in order to come to some preliminary estimate of their efficiency. I have noted with some surprise that many psychotherapists have refused to use such methods as conditioning therapy in enuresis, not on empirical grounds, but on *a priori* grounds, claiming that such mechanical methods simply could not work, and disregarding the large body of evidence available. Even in long-established sciences *a priori* considerations carry little weight; in such a young discipline as psychology they are quite out of place. Only actual use can show the value of one method of treatment as opposed to another.

There is one point I would like to emphasize. Freud developed his psychological theories on the basis of his study of neurotic disorders, and their treatment. Behaviour therapy, on the contrary, began with the thorough experimental study of the laws of learning and conditioning in normal people, and in animals; these well-established principles were then applied to neurotic disorders. It seems to me that this latter method is in principle superior to the former;

scientific advance has nearly always taken the form of making fundamental discoveries and then applying these in practice, and I can see no valid reason why this process should be inverted in connection with neurosis. It may be objected that learning theorists are not always in agreement with each other (24), and that it is difficult to apply principles about which there is still so much argument. This is only very partially true; those points about which argument rages are usually of academic interest rather than of practical importance. Thus reinforcement theorists and contiguity theorists have strong differences of view about the necessity of reinforcement during learning, and different reinforcement theorists have different theories about the nature of reinforcement. Yet there would be general agreement in any particular case about the optimum methods of achieving a quick rate of conditioning, or extinction; these are questions of fact, and it is only with the interpretation of some of these facts that disagreements arise. Even when the disputes about the corpuscular or wavelike nature of light were at their height, there was sufficient common ground between contestants regarding the facts of the case to make possible the practical application of available knowledge; the same is true of learning theory. The 10 per cent. which is in dispute should not blind us to the 90 per cent. which is not—disagreements and disputes naturally attract more attention, but agreements on facts and principles are actually much more common. Greater familiarity with the large and rapidly growing literature will quickly substantiate this statement (38).

It is sometimes said that the model offered here differs from the psychoanalytic model only in the terminology used, and that in fact the two models are very similar. Such a statement would be both true and untrue. There undoubtedly are certain similarities, as Mowrer (35) and Miller and Dollard (5) have been at pains to point out. The motivating role of anxiety in the Freudian system is obviously very similar in conception to the drive-producing conditioned autonomic responses of learning theory, and the relief from anxiety produced by hysterical and obsessional symptoms in Freudian terminology is very similar to the conditioned drive-reducing properties of motor movements. Similarly, a case could be made out in favour of regarding the undersocialized, non-conditionable psychopathic individual as being Id-dominated, and the dysthymic, over-conditionable individual as being Super-Ego dominated. Many other similarities will occur to the reader in going through these pages, and indeed the writer would be the first to acknowledge the tremendous service that Freud has done in elucidating for the first time some of these dynamic relationships, and in particular in stressing the motivating role of anxiety.

Nevertheless, there are two main reasons for not regarding the present formulation as simply an alternative differing from the psychoanalytic one only in the terminology used. In the first place, the formulation here given differs from the Freudian in several essential features, as can be seen most clearly by studying Table I. Perhaps these differences are most apparent with respect to the deductions made from the two theories as to treatment. Psychoanalytic theory distrusts purely symptomatic treatment and insists on the removal of the underlying complexes. Behaviour theory on the other hand stresses the purely symptomatological side of treatment and is unconvinced of the very existence of "complexes". It might, of course, be suggested that there is some similarity between the Freudian "complex" and the "conditioned surplus autonomic reaction" posited by behaviour theory. That there is some similarity

cannot be denied, but no one familiar with psychoanalytic writings would agree that the Freudian complex was not in essence a very different conception from the conditioned autonomic response, both from the point of view of its origins, as well as from the point of view of the appropriate method of extinction.

This brings me to the second great difference between the two models. What the Freudian model lacks above all is an intelligible objectively testable *modus operandi* which can be experimentally studied in the laboratory, which can be precisely quantified, and which can then be subjected to the formulation of strict scientific laws. The stress on such a mechanism, namely that of conditioning, is the most noteworthy feature of the model here advocated. It is entirely due to the great body of research which has been done in connection with the elaboration of laws of modern learning theory that we are enabled to make fairly precise deductions resulting in different methods of treatment for patients suffering from neurotic disorders, and it is with respect to this feature of the model that the relevant case histories and accounts of treatment should be read (28, 33, 47).

It has sometimes been suggested that the criticisms which I have levelled against the psychotherapeutic schools because of their failure to provide adequate control groups to validate their claims regarding the curative properties of their methods, could justifiably be levelled against the accounts given by those who have used behaviour therapy and reported upon the effects achieved. Such a criticism would not be justified for two reasons. In the first place the cases quoted are *illustrative of methods*, not *proofs of psychotherapeutic efficacy*; the only case in which claims regarding relative efficacy have been made contains a statistical comparison with the effects of psychoanalytic treatment of similar cases (46). In the second place the concept of "control" in scientific experiments is somewhat more than simply the provision of a control group; the control in an experiment may be *internal*. As an example, consider the experiment reported by Yates (47) on the extinction of four tics in a female patient by means of a rather novel and unusual method, namely that of repeated voluntary repetition of the tic by massed practice. Precise predictions were made as to the effects that should follow, and these predictions were studied by using the fate of some of the tics as compared to the fate of other tics submitted to dissimilar treatment. Thus, practice for two tics might be discontinued for a fortnight, while practice on the other two would go on. By showing that the predictions made could thus be verified, and the *rate of extinction* of the tics varied at will in accordance with the experimental manipulation for such variables as massing of practice, a degree of control was achieved far superior to the simple assessment of significance produced in the comparison of two random groups submitted to different treatments. It is by its insistence on such experimental precision and the incorporation of experimental tests of the hypotheses employed, even during the treatment, that behaviour theory differs from psychotherapy.

There is one further method of pointing up the differences between the two theories and of deciding between them; I mention this matter with some hesitation because to many psychiatrists it seems almost sacrilegious to use animal experimentation in the consideration of human neurosis. However, Fenichel himself (18, p. 19) has quoted "experimental neuroses" as support for the Freudian conception of neurotic disorders, and it is with respect to these experiments that the contrast between the psychoanalytic and our own model may be worked out most explicitly. Fenichel maintains that the model of

psychoneurosis "is represented by the artificial neuroses that have been inflicted upon animals by experimental psychologists. Some stimulus which had represented pleasant instinctual experiences or which had served as a signal that some action would now procure gratification is suddenly connected by the experimenter with frustrating or threatening experiences, or the experimenter decreases the difference between stimuli which the animal had been trained to associate with instinct gratification and threat respectively; the animal then gets into a state of irritation which is very similar to that of a traumatic neurosis. He feels contradictory impulses; the conflict makes it impossible for him to give in to the impulses in the accustomed way; the discharge is blocked, and this decrease in discharge works in the same way as an increase in influx; it brings the organism into a state of tension and calls for emergency discharges.

"In psychoneuroses some impulses have been blocked; the consequence is a state of tension and eventually some 'emergency discharges'. These consist partly in unspecific restlessness and its elaborations and partly in much more specific phenomena which represent the distorted involuntary discharges of those very instinctual drives for which a normal discharge has been interdicted. Thus we have in psychoneuroses, first a defense of the ego against an instinct, then a conflict between the instinct striving for discharge and the defensive forces of the ego, then a state of damming up and finally the neurotic symptoms which are distorted discharges as a consequence of the state of damming up—a compromise between the opposing forces. The symptom is the only step in this development that becomes manifest; the conflict, its history, and the significance of the symptoms are unconscious".

Hebb (22) has laid down certain requirements for attempting to demonstrate that experimental neurosis occurs in animals and Broadhurst (2, 3) has examined the literature, and particularly that referred to by Fenichel, from this point of view. Here is his summary.

"How does the large body of American work stand up to such an assessment? For the purposes of a recent review (3), the available literature was examined in the light of Hebb's criteria. Noteworthy among this is the work of the group headed by Liddell (1), one of the pioneers of conditioning methodology in the United States, who has used principally the sheep as his experimental subject; of Gantt (20), whose long term study of the dog 'Nick' is well known; and of Masserman (30), who has done extensive work using cats. This is not the place to enter into the details of this evaluation, which is reported elsewhere (3), but the overall conclusion which was reached was that there are few instances in all this work of any cases of experimentally induced abnormalities of animal behaviour which meet all of Hebb's criteria. Let us take, for example, the work of Masserman, whose theoretical interpretation of abnormal behaviour need not concern us here except to note that it was the basis upon which he designed his experiments to produce "conflict" between one drive and another. What he did was this. He trained hungry cats to respond to a sensory signal by opening a food box to obtain food. Then he subjected them to a noxious stimulus, a blast of air, or electric shock, just at the moment of feeding. The resulting changes in behaviour—the animals showed fear of the situation and of the experimenter, and refused to feed further—he identified as experimental neurosis. But the behaviour observed fails to fulfil more than one or two of Hebb's criteria, and, moreover, certain deficiencies in the design of his experiments make it impossible to

draw any satisfactory conclusions from them. Thus Wolpe (45) repeated part of Masserman's work using the essential control group which Masserman had omitted—that is, he gave the cats the noxious stimulus alone, without any “conflict” between the fear motivation thus induced, and the hunger which, in Masserman's animals, operated as well—and found that the same behaviour occurred. It hardly needs to be said that a fear response to a threatening stimulus is not abnormal and cannot be regarded as an experimental neurosis.”

It is clear from the studies cited that Fenichel is quite wrong in claiming that “experimental neurosis” is in any way analogous to the Freudian model of human neurosis. It appears, therefore, that in so far as these studies are relevant at all they can be regarded as demonstrating nothing but simple conditioned fear responses of the kind called for by our theory. It is perhaps worthy of note that the failure of psychoanalysis to use control groups in the human field has extended to their work with animals, as in the case of Masserman quoted above. Fenichel's easy acceptance of data congruent with his hypothesis is paralleled by his failure to mention data contrary to the psychoanalytic viewpoint. By taking into account all the data it seems more likely that a correct conclusion will be reached.

I would now like to return to some of the points which I raised at the beginning of this paper. I argued then that the special knowledge and competence of psychologists in mental hospitals was largely wasted because of concentration on, and preoccupation with, Freudian theories and projective types of test. I would now like to make a more positive suggestion and maintain that by virtue of their training and experience psychologists are (or should be) experts in the fields of conditioning and learning theory, laboratory procedures, and research design. In suitable cases, surely their help would be invaluable in diagnostic problems, such as ascertaining a given patient's speed of conditioning, in the theoretical problem of constructing a model of his personality dynamics, and in the practical problem of designing a suitable course of behaviour therapy which would take into account all the available information about the case.* I am not suggesting that psychologists should themselves necessarily carry out this course of treatment; it would appear relatively immaterial whether the therapy is carried out by one person or another, by psychologist or psychiatrist. Both types of procedure have been experimented with, and both have shown equally promising results. Indeed, certain aspects of the therapy can obviously be carried out by less senior and experienced personnel, provided the course of treatment is reviewed periodically by the person in charge. Psychoanalysis lays much stress on what is sometimes called “transference”, a devil conjured up only to be sent back to his usual habitat with much expenditure of time and energy (18). Behaviour therapy has no need of this adjunct, nor does it admit that the evidence for its existence is remotely

* It will be clear that the function here sketched out for the psychologist demands that he be furnished with the necessary tools of his trade, such as sound-proof rooms, conditioning apparatus, and all the other techniques for delivering stimuli and measuring responses on a strictly quantified basis (13). It is equally clear that such facilities do not exist in the majority of our mental hospitals. Until they do, the handicaps under which the clinical psychologist works at such institutions will be all but insurmountable, and no reasonable estimate of their potential usefulness can be formed. One might just as well employ an electroencephalographer and refuse to pay for the machine which he has been trained to use! It would be better to have a few, properly equipped departments than a large number of small, ill-equipped ones as at present. Even in the United States the position is bad; in this country it is worse. A relatively small capital investment would be likely to bear considerable fruit.

adequate at the present time. However that may be, relinquishing the personal relationship supposed to be indispensable for the "transference" relation allows us to use relatively unqualified help in many of the more time-consuming and routine parts of behaviour therapy. In certain cases, of course, personal relationships may be required in order to provide a necessary step on the generalization gradient; but this is not always true.*

From a limited experience with this kind of work, carried out by various members of my department, I can say with confidence two things. The direct application of psychological theories to the practical problem of effecting a cure in a particular person, here and now, acts as a very powerful challenge to the psychologist concerned, and makes him more aware than almost anything else of the strengths and weaknesses of the formulations of modern learning theory. And the successful discharge of this self-chosen duty serves more than almost anything else to convince his psychiatric colleagues that psychology can successfully emerge from its academic retreat and take a hand in the day-to-day struggle with the hundred-and-one problems facing the psychiatrist. It seems to me that the tragic fratricidal struggle between psychiatrists and psychologists, which has so exacerbated relations between them in the United States, could easily be avoided here by recognizing the special competence of the psychologist in this particular corner of the field, while acknowledging the necessity of keeping the general medical care of the patient in the hands of the psychiatrist. I believe that most psychiatrists are too well aware of the precarious state of our knowledge in the field of the neurotic disorders to do anything but welcome the help which the application of learning theory in the hands of a competent psychologist may be able to bring.

* As an example of this we may quote a case reported by Graham White. This concerns a child who became anorexic after the death of her father. The therapist adopted the father's role in a variety of circumstances, ranging in order from play with dolls' teasetts to the actual eating situation, and reinforced those reactions which were considered desirable. The theoretical rationale was that the father had become a conditioned stimulus on which eating depended.

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THE INHERITANCE OF NEUROTICISM: A REPLY

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THIS is a reply to a critique by Karon and Saunders (9) of the Eysenck and Prell work on the inheritance of neuroticism (6). This paper is a curious combination of sophisticated statistical analysis and argument by irrelevant association. Thus the authors mention the fact that we found a strong heredity predisposition but go on to say that "the results are so much at variance with general clinical experience that doubts arise in the minds of many psychologists . . . particularly those who have investigated, in a therapeutic situation, the source and development of neurotic reactions". The authors do not indicate how it is possible through general clinical experience or in the therapeutic situation, to find an answer to one of the most difficult and complex problems in the whole field of psychology. Whether neurotic predisposition is largely inherited or has little hereditary basis, would not seem to be capable of being discerned simply by giving psychotherapy to a few neurotics.*

Again the authors conclude that "the possible implications of the Eysenck-Prell study are of great practical importance to the practising clinician"; Karon and Saunders come to this conclusion because some unspecified "proponents of heredity" apparently imply that we should act as if there were some necessary relationship between hereditary determination and invulnerability to therapy. No such view has ever been held by either of us and, therefore their whole argument is quite irrelevant to the critique of our experiment. The efficacy or otherwise of psychotherapy and physiotherapy in neurotic disorders must be demonstrated empirically; it cannot be deduced from any "higher principles". In any case, the hypothesis that predisposition to neurotic breakdown is largely inherited does not carry any implications one way or another with respect to the possibilities of cure or prophylaxis.

Karon and Saunders go on to criticize us for faulty reporting; they do this on the ground that 68 pairs of twins were located but only 50 pairs were tested and their results analysed. They imply that 18 cases were discarded and revert again and again to hypothetical reasons for dropping these 18 cases. There is no basis here for any such criticism, however, as our report makes clear that

* This point will be obvious when we consider the kind of evidence required. In the first place, we would need to study unselected or random groups of subjects; by definition the group of people subjected to psychotherapy is heavily weighted in the abnormal, neurotic direction, and can hardly qualify as *random*. In the second place, we must have some criterion to distinguish between hereditary and environmental influences. The often noted similarity between parents and children with respect to intelligence, neuroticism, etc., is clearly not such a criterion as it is equally explicable in terms of hereditary and environmental influences. Karon and Saunders fail to indicate how the practising psychiatrist can acquire such a criterion or use it in his day-to-day work. In the best of my knowledge, no such simple criterion exists. Karon and Saunders' appeal appears to be to prejudice rather than to fact. An analogy might be the appeal to "simply look around" of the supporters of the geocentric and flat-earth theories, when criticizing Copernicus and Galileo; this appeal also is irrelevant as both sets of theories would equally well explain the phenomena obvious to common sense.

58 pairs of twins were *located*; not all of them, however, were tested. We settled on 25 identical and 25 fraternal twins at the outset of the experiment; we anticipated that in a number of cases permission to test would not be granted, or that apparatus might break down in the middle of testing. We, therefore, located a larger number of cases in order to have reserves in hand. As it turned out, however, permission was not refused for any of the twins approached and the apparatus did not break down. Consequently, it did not become necessary to call upon any of the remaining 18 twins. The criticism that we eliminated certain cases after testing for some sinister and mysterious reason as implied by Karon and Saunders cannot, therefore, be maintained.

An interesting feature of our study was the fact that as stated in our paper "in 16 out of 18 cases the identical twin variance is larger; two of these differences are significant at the 2 per cent. level". Karon and Saunders use this fact to criticize our calculation of Holzinger's h^2 (11, 10) and present an alternative value which would reduce the hereditary determination of neuroticism to 58 per cent.* The question they raise is an important one. The concept of statistical significance is, of course, an arbitrary one; to say that a difference is significant at the 5 per cent. level simply means that it would have arisen by chance in only one case out of twenty. Many people prefer a higher significance level such as $p = .01$, i.e. the probability of a given difference having arisen by chance is only one in a hundred. The particular value chosen depends in part on the *a priori* probability of a given event; thus psychologists have quite rightly demanded much higher probabilities in the case of extra-sensory perception where the *a priori* probabilities are very much against the occurrence of such events, than is usual in more orthodox research. Now as Karon and Saunders point out, it would have been easier to understand and rationalize the occurrence of a higher variance for the *fraternal* than for the *identical* group. The opposite finding has a very low *a priori* probability and would, therefore, in my submission require a higher probability value than five per cent. for its substantiation; indeed anything below the one per cent. level would hardly be regarded as more than suggestive. Under the circumstances we felt that on replication of the experiment it would be unlikely that the observed difference would be found again and we decided, therefore, to regard it as accidental. We were confirmed in this decision by the fact that two of the best neuroticism tests (autokinetic movement and suggestibility) had very high h^2 values (.648 and .701) although variances were not significantly different.

It may be helpful to invert the argument. If we had claimed as a demonstrable fact that variances for identical twins were greater than the variances for fraternal twins ($i\sigma^2 > f\sigma^2$), simply because in one study this highly unlikely inequality had been found, we would rightly have been criticised for over-interpretation. Taking all the known facts into account, i.e. refusing to use blindly an arbitrary criterion of "significance", we would conclude that the data did not disprove the null hypothesis ($i\sigma^2 = f\sigma^2$) at a level acceptable to us. If on repetition a similar result to ours should be found, we would of course have to revise our conclusion; it is our belief that repetition of the experiment would not duplicate this particular finding.

* They conclude rather mysteriously that "under conditions where much if not most environmental variance is held constant, only about 30 per cent. of a crude neuroticism criterion may be attributed to hereditary determinants". They do not explain how they arrive at this figure, which seems to owe little to mathematical calculation and much to rather unwarranted guesswork. Such speculations do not appear to further the scientific study of genetics as applied to human behaviour.

It must be freely admitted that our case rests on an argument which may not appeal to all statisticians; indeed, there is much controversy on this precise point. On reflection it is still my opinion that our conclusion is preferable to that arrived at by Karon and Saunders. It was our hope when originally writing this paper, that others would repeat the experiment with larger samples and better tests than we had available at the time. Until such repetition is in fact carried out, it is impossible to arrive at a decision between the two alternative methods of analysis which cannot be criticized statistically. (It should be noted, of course, that even if we accept Karon and Saunders' figure, we would still find a substantial hereditary contribution to neuroticism; thus even on their showing our main contention would still receive support.)

Karon and Saunders go on to question the validity of our neuroticism criterion. I find it impossible to follow their argument. They quote in a table the biserial correlations between tests and criterion as well as the h^2 of each test, and comment that "we cannot judge from these data whether a better 'neuroticism' factor could be extracted from the battery". This is hardly surprising because the figures in this table by themselves cannot, of course, be used in that fashion. As explained in the article, the factors originally obtained were rotated until the new factor 1 achieved maximum correlation with the criterion column. It is this method of "criterion analysis" (2) which ensures that no better neuroticism factor could be extracted from the battery, and the table quoted by Karon and Saunders is quite irrelevant.

Karon and Saunders make several further comments, some of which are justified, some not. They point out that the differences between the normal twins and the neurotic criterion children "include any differences inherent in *twins versus non-twins* as well as between normals and neurotics". This is true, but not in our opinion important. The point might have had more weight if we had selected our tests simply on the basis of the differentiating power between the twins and the neurotics. By interposing a factor analysis and using the factor score, we have made it much less likely that the twin-non-twin difference would affect our results. The other point made by Karon and Saunders is that the differences between normals and neurotics "are further blurred by the fact that the 'normal' sample of twins must contain some neurotics; indeed if it does not this sample cannot contain variance associable with neuroticism and the whole experimental design collapses". This argument is quite erroneous. It is equivalent to saying that in comparing a group of tall and a group of small people, that the latter group must have contained some giants as otherwise there would be no variance relating to height. Neuroticism is regarded as a quantitative variable and there is no need for the neurotic group to overlap with the normal, although it is, of course, quite likely that in actual fact the groups did overlap. The use of this argument which implies that "neurotics" are a categorical group discontinuous with normals makes me feel that Karon and Saunders have not properly understood the underlying logic of our experiment.

A last point made by Karon and Saunders relates to the selection of tests. Looking at the h^2 values for the tests they say that "these figures provide a striking impression that the tests might just as well have been chosen on the basis of having a high hereditary determination . . . it is quite clear that *any* factor extracted is likely, because of the nature of the tests, to be heavily weighted towards the side of heredity" and in their summary they say that "the validity of the neuroticism criterion itself is . . . biased in favour of

hereditary components through the selection of tests". It is difficult to understand the basis of this criticism. The tests were chosen because on the basis of previous work we considered them to be possible measures of neuroticism;* we did not know for any of these tests whether performance on them would or would not be determined to any great degree by heredity. Some of the tests succeeded in measuring neuroticism; others failed. The neuroticism factor extracted from the whole battery segregated neurotic and normal children reasonably well (7), so that we may conclude that our original choice, though far from perfect, was not unreasonable. The fact that the tests did in fact have relatively high h^2 values and consequently showed evidence of strong hereditary determination, may go counter to Karon and Saunders environmentalist beliefs but it is difficult to see how this affects the issue. Indeed, the longer I study the argument the less I understand precisely what they are trying to prove. What they say would only be relevant if we had had available a large battery of tests, the hereditary determination of each of which was already known, and had then with malice aforethought picked out all those tests having high h^2 values while leaving out those with low h^2 values. As we had no *a priori* knowledge of this kind we could not have carried out such a programme even should we have wished to do so.

Having disagreed with almost every point brought forward in criticism by Karon and Saunders, I would like to end by saying how much I agree with their final conclusion. They say "only our efforts to understand determining mechanisms seem likely to produce successful attempts to remedy or prevent situations [*sic*] of any sort including the neuroses. Such understanding of the underlying order in the universe is the proper goal of scientific investigation". I take this to mean that we should try to study in greater detail the precise method of hereditary determination of neuroticism, rather than rest content with a simple numerical estimate of its contribution. Such investigations, making use of the most recent methods of polygenic analysis, are at the moment being carried out in our laboratory, including the method of "diallel crossing". These may succeed in giving a more direct proof of the action of genetic forces, as well as estimates of linkage and dominance. Until these studies are completed it would seem useful, however, to repeat the Eysenck-Prell study with suitable technical improvements, in order to throw some further light on the relative importance of the factors in question. My belief that such studies would support the result of the original paper is increased considerably by the fact that in recent work from the Genetics Unit of the Institute on identical twins brought up in separation, sizable correlations were found for both neuroticism and extraversion, as well as for intelligence (12); indeed, the order of size of the correlations, bearing in mind the reliabilities of the tests, were such that our own finding of similar degrees of hereditary determination for personality traits as for intelligence might be considered supported (4).

*Karon and Saunders, in discussing our selection of tests, complain that the basis for deciding on the particular battery chosen "is nowhere made clear by Eysenck and Prell". We relied on the results of work with children currently proceeding then by Himmelweit and Petrie (8), Connor (1), Thorpe (13) and others, as well as our own experimental studies of neuroticism in adults (2, 3). Karon and Saunders do not suggest how our selection could have been improved on the basis of knowledge available then; at the present moment it is not impossible that a better set of tests could be selected, partly due to recent work on perceptual indicators of neuroticism (5), and partly to the linking-up of learning phenomena with the same personality variable (4). But this knowledge was not then available.

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EXPRESSIVE MOVEMENT AND LEARNING: A STUDY OF SCORE COMPLEXITY

By

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INTRODUCTION

THE present report deals with two effects of movement variability as a function of practice in the drawing from memory of visually presented patterns. The first effect is found in initial practice stages only and appears to be related to the neurotic dichotomy of hysteria/dysthymia*. The second effect is specific to late practice stages and related to schizophrenics as against neurotics and controls. Results are mainly discussed to illuminate the problem of score complexity in motor learning. The aim is to show how, in learning involving motor activity, the extent and variability of movement components may affect the validity of learning scores in their relation to personality.

Jones (10), in his review on learning in abnormals, has noted considerable discrepancies between experiments employing different techniques. These differences in learning efficiency were largely explained in terms of individual differences in the reaction to varying degrees of "stress" associated with the different tasks. Theoretical models were constructed accordingly. This is one procedure to disentangle the reported discrepancies. An alternative approach, quite apart from the problems of difficulty, stressfulness and type of task, is to analyse whether the various learning scores used are in fact psychologically comparable. To quote an example from Jones' review, extraverts, as against introverts, were found to be inferior on certain learning tasks but superior on others. Before accepting this as a true result the possibility should be scrutinized that, dependent on the task employed, various forms of behaviour other than learning proper may affect the learning score to such an extent as to render results contradictory. If this is the case, the setting up of corresponding theoretical models appears premature.

The justification for this argument is derived from the following facts. In a number of pursuit-rotor experiments, performed in the Maudsley Hospital as quoted by Jones (10), correct performance (contact with the target disc) was consistently better for introverts than for extraverts, though predominantly not significantly so. The same result was obtained by the present author when using a motor task of tracing a target line while wearing prismatic inversion lenses (2). Essentially identical results were thus obtained despite some considerable variations in tests and method of presentation. On the surface, they tend to confirm Eysenck's extraversion theory on learning, which claims that amongst other variables rate of performance correlates negatively with extraversion (9).

At this point it should be borne in mind that learning efficiency was studied

* The terms hysteria/dysthymia and extraversion/introversion are used in accordance with Eysenck's theory of extraversion (8), the former representing the neurotic variants of the normal dimension of extraversion/introversion.

through the medium of motor activity. This is of cardinal importance as the present author has, on a number of occasions, established the fact that movement extent and particularly movement variability are significantly related to extraversion (1, 2, 5, 6). As it is likely that in motor tracking tasks both movement extent and variability cause relatively poor or shortened contact with the target, the above stated inferior motor performance of extraverts may have resulted from their particular movement characteristics rather than from learning *per se*. This view is considerably strengthened by two facts. In the above-mentioned tracing experiment, with subjects wearing prismatic lenses (2), care was taken to analyse both correct tracing (on target) as well as error tracing (extent of movement off the target). While the amount of correct tracing was somewhat smaller for extraverts than for introverts, the amount of error tracing was very significantly larger. It follows from this that increase in target size leads to inclusion of more of the extraverted type of extensive off-target movement into the learning score until the result is reversed, i.e. extraverts perform superior provided the target is sufficiently large.

While further analysis of this problem is in preparation, an essentially identical analysis, using a very different type of task, is dealt with in the present paper. This task permits scoring of error in terms of several types of deviations from the stimulus found in drawn reproductions. A first analysis of learning an extremely difficult task has already shown that normal and abnormal extraverts performed preponderantly better than introverts, though not significantly so (3). Using a considerably easier task under short exposure condition—which, incidentally, proved to be specific to extraversion in contrast to long exposure—extraversion in neurotics correlated positively and very significantly with superior performance (4). The three error scores used in these analyses were designed to exclude movement extent and variability from interfering with the learning score.

The question is now asked whether, on re-analysis of the experiment in question (3) so as to permit movement characteristics of the type discussed to affect the learning score, extraverts do in fact perform worse than introverts. If so, considerable doubt is thrown on the feasibility of relating most of the known types of motor learning to extraversion. The problem may now be defined in a more specific manner. The rotary pursuit and motor tracing experiments, referred to above, have consistently shown that increase in rate of performance of extraverts is inferior to that of introverts in early stages of practice. With prolonged practice, means of these two subgroups converge until they are virtually identical. The present analysis considers this effect by expecting that differences with respect to a complex movement/learning score between extraverts and introverts are largest in early stages of learning and diminish steadily with practice.

The general hypothesis underlying these arguments is that extraversion correlates negatively with rate of learning if certain style of movement is allowed to interfere with learning performance proper. If scored without such interference, learning is not negatively related to extraversion regardless of the difficulty level of the task employed. Instead, provided certain medium levels of difficulty and/or certain exposure conditions are used, extraversion will correlate positively and significantly with learning. Under other conditions it will not correlate significantly while other personality traits will be of greater importance.

As was stated in the beginning, a second effect specific to late practice stages and to psychotics, in the main schizophrenics, is reported. This was

observed empirically and without anticipation. However, as it was successfully cross-validated the report of this effect appears justified.

METHOD

1. *The Figure Reconstruction Test (FRT)*

The main score used in the present analysis is termed *distance variability*. It is the fourth error score derived from the Figure Reconstruction Test (FRT), a pattern of which is seen in Figure 1.

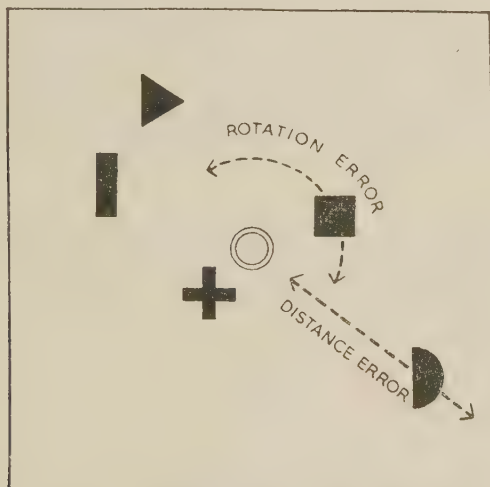


FIG. 1.—FRT pattern demonstrating axial (rotation error) and radial (distance error) error types. Two patterns of this kind, successively exposed, formed the learning stimulus of the experiments described.

On previous occasions, two such patterns were exposed consecutively and reproduced on two separate response sheets of 31 cm. square. The actual test shapes (semi-circle, triangle, square, oblong and cross) had to be drawn around the central reference point used for scoring only. Twelve such trials were asked for in a first experiment (Exp. I) as against twenty trials in a second experiment (Exp. II). Drawn reproductions may be analysed for deviation from the stimulus in terms of error axial (rotation error) and radial (distance error) to the central reference point. Method and results have been described fully in an earlier publication (3).

2. *Description of Error Scores Uncontaminated by Expressive Movement*

We now describe three of the four error measures. The first three were designed to prevent individual variation in the area covered in drawing and the respective variability in area between successive drawings to contaminate the error score. The three scores are:

Rotation Error: Mean axial displacement of the individual stimulus elements, measured in degrees.

Rotation Variability: Change of rotation error between successive reproductions in degrees, disregarding the direction of change.

Distance Error: Radial, either centrifugal or centripetal, displacement of test shapes, measured in terms of number of places away from the stimulus position.

The first two scores, dealing with displacement about the centre, are, for logical reasons, quite unlikely to be influenced systematically and significantly by movement extent and variability. Radial error, however, will be greatly affected by such movement characteristics unless special precaution is taken. In the stimulus the distance of the five test shapes from the centre increases arithmetically. From the drawings the reproduced distances are determined first and then compared with the stimulus order. Distance error is scored in terms of discrepancy between stimulus and reproduced orders, or in terms of number of places a shape was reproduced away from the stimulus order. This operation completely rules out effects due to extent of movement and greatly reduces effects of movement variability. This has already been discussed elsewhere (6, Table IV) and the correlations quoted in Table I demonstrate the effected high degree of independence of error from movement.

3. Description of Expressive Movement Scores Uncontaminated by Error

The four movement scores shown were scored so as to minimize the effect of error on movement. They are defined as follows:

Distance: mean distance of reproduced shapes per trial from the centre of the reproduction sheet.

Distance variability between pattern means (DV pattern): variability between distance means of patterns, or, the mean deviation of a given number of distance scores from their mean.

Distance variability within ranges (DV ranges): identical to the preceding score, except for measuring separately at each of the five distances, or ranges, at which Ss choose to draw.

Distance spread: mean deviation of shapes from the mean distance within trials.

Correlations between movement and error scores are now shown in Table I.

TABLE I

Extent and Variability of Movement in Drawings Correlate Lowly with Error Scores Designed to be Uncontaminated by such Movement Characteristics

Correlations of Expressive Movement with Error Scores				Distance	Spread	DV Pattern	DV Ranges
Rotation	I	..	..	0.23*	0.09	0.24*	0.19
Error	II	..	..	-0.21	-0.13	0.19	0.20
Rotation	I	..	..	0.24*	0.13	0.09	0.22*
Variability	II	..	..	0.06	-0.08	0.34†	0.41†
Distance	I	..	..	0.01	-0.20*	0.18	0.16
Error	II	..	..	-0.07	-0.07	0.08	0.08

Exp. I=100 neurotics, II=67 normals, neurotics and schizophrenics.

Levels of significance: * = 5 per cent., † = 1 per cent.

The general picture emerges that extent of movement (distance and spread) is inconsistently related to error. The coefficients for the two variability scores (distance variability for pattern and ranges) are consistently positive but low

the average. It is concluded that the first three error scores used are essentially independent of the movement characteristics present during reproduction.

4. *Description of a Complex Expressive Movement/Learning Error Score**

We now turn to the fourth error score, termed *distance variability (error)*, designed to be a complex score made up of movement and error components. This score is defined as the change in mms. between successive trials of the distance from the centre, at which the individual shapes are drawn. It includes extent of movement. Variability of distance was expected to reflect both movement/variability *per se* and uncertainty as to the correct placement of test shapes (error). This expectation was fully borne out, as demonstrated further below, and distance variability may justifiably be used as a complex score. The choice of variability over the extent (distance) of movement was necessitated by the fact that the latter does not display systematic practice effects as expected from an error score (6, Fig. 2).

5. *Subjects*

The scores described are analysed with respect to the samples quoted below and employed in two experiments.

					Experiments	
					I	II
Normal controls	..	..	..	..	40	22
Neurotics	..	..	..	..	100	30
Hysterics	..	..	..	..	39	15
Dysthymics	..	..	..	..	61	15
Psychotics	..	..	..	..	28	15

A detailed description of these samples has already been given (3). Suffice it to say that groups were equated for age and intelligence and that there were no gross differences with regard to social status. In I, an unselected sample of neurotics was used; in II, in accordance with Eysenck's theory of extraversion, only clear-cut cases of hysterics and dysthymics were chosen, using pooled clinical ratings and Guilford's Rathymia score as criteria. The psychotics of both experiments were all schizophrenic except for seven endogenous depressions. In II, they were all schizophrenics.

RESULTS

1. *Distance Variability as Related to Hysteria/Dysthymia*

Results on distance variability are first related to the dichotomy of hysteria/dysthymia, as shown in Figure 2.

Large practice effects are seen in all groups suggesting that we are dealing with error curves. The hysterics scored higher than the dysthymics throughout, whereas the psychotics and controls, with the exception of late practice stages, assumed an intermediate position. Furthermore, differences between the means

* As points 3 and 4 show, there are two types of distance variability scores. The first type is scored disregarding the identity of test shapes. For "DV pattern" the correct location on the response sheet of the individual shapes is irrelevant. The same is true for "DV ranges", where the reproduced ranges were used regardless which test shape was placed on a particular range. Error is thus automatically excluded. In the case of distance variability (error), test shape identity is regarded. The question is now how much each particular shape varies its position between successive trials.

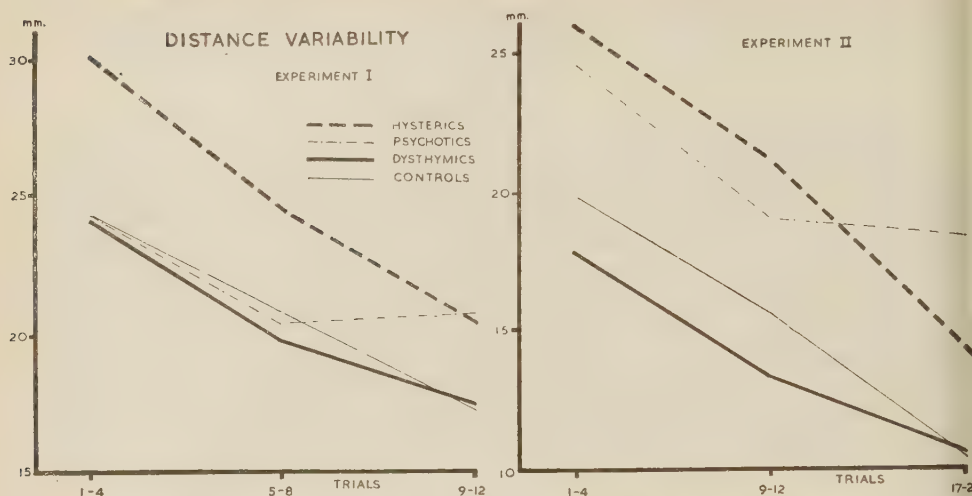


FIG. 2.—Distance variability, an error score contaminated by movement extent and variability, is in early practice stage related to hysteria/dysthymia (primary variability). With prolonged practice psychotics tend to score highest (secondary variability). In I three-quarters and in II all of the psychotics are schizophrenics.

of the hysterics and dysthymics decreased with practice. The significance of the differences between the latter two groups was tested by means of analysis of variance, as shown in Table II.

TABLE II

Analysis of Variance Between the Hysterics and Dysthymics Separated for Three Blocks of Trials

Experiment I	d.f.	s.sq.	m.sq.	F-ratio	Significance (Per cent.)
Between trials ..	2	3,161.447	1,580.724	20.256	0.1
Between groups ..	1	1,465.479	7,465.479	18.779	0.1
Interaction T × G ..	2	99.141	49.571	—	N.S.
Between classes ..	5	4,726.067	—	—	—
Within classes ..	294	22,943.600	78.039	—	—
Total ..	299	27,669.667	—	—	—
Experiment II					
Between trials ..	2	760.424	380.212	6.858	1
Between groups ..	1	378.000	378.000	6.817	5*
Interaction T × G ..	2	707.400	353.700	6.379	1
Between classes ..	5	1,845.824	—	—	—
Within classes ..	84	4,657.332	55.444	—	—
Total ..	89	6,503.156	—	—	—

Nearly 1 per cent.

The “between trials” analysis shows that highly significant practice effects occurred, which supports the view that distance variability may be classified as an error variable. The “between groups” variation is almost equally significant for both experiments and permits the conclusion that distance variability is both reliably and significantly higher for hysterics than for dysthymics. With

these results the first expectation is borne out, viz.: extraverts (hysteries) score inferior to introverts (dysthymics) provided a complex error score is used containing both movement variability and a measure of error proper. In contrast to this, the same extraverts tended to score better on three learning measures, discussed in the method section, uncontaminated by movement variability and derived from the same experiment (compare 3). These results had not been significant.

2. *The Dependency of the Movement Effect on Practice*

Table II reveals furthermore that the interaction term for I is insignificant but significant at the 1 per cent. level for II. This indicates that the postulated decrease of the differences obtained between the hysteries and dysthymics depends on the amount of practice given, which was greater for II. This effect is more closely examined by means of critical ratios between the group means which were computed using the "within classes" variance of the analysis of variance tables. Corresponding results are presented in Table III.

TABLE III

Differences in Distance Variability Between Hysteries and Dysthymics Decrease with Practice

Trials: Experiment I				1-4	5-8	9-12
Means	{ Hysteries			30.077	24.231	20.487
	{ Dysthymics			24.164	19.590	17.443
t-ratio	..	..	..	3.261	2.560	1.679
Significance	..	..	..	1%	2%	N.S.
Trials: Experiment II				1-4	9-12	17-20
Means	{ Hysteries			26.133	21.067	14.333
	{ Dysthymics			17.800	13.333	10.400
t-ratio	..	..	..	3.065	2.844	1.446
Significance	..	..	..	1%	1%	N.S.

D.F.: I=294, II=84.

The consistent result emerges that differences in distance variability between hysteries and dysthymics are largest in initial practice stages and decrease with practice.

3. *Distance Variability as Related to Psychosis*

Experiment I revealed that psychotics, after scoring relatively low in the beginning, tended to form a plateau and to equal the mean score of hysteries during late practice stages (Fig. 2). This result was confirmed in II. Moreover, and apparently due to the increased number of trials, the schizophrenic mean was now higher than that of the hysteries during the last block of trials. The significance of these results is assessed in Table IV.

The upper portion of Table IV shows that hysteries and psychotics (mainly schizophrenics) did not differ significantly as regards distance variability at the second trial block. With further practice, as measured by the difference between the second and third trial blocks, the psychotics improved hardly at all, whereas the hysteries improved significantly over the psychotics. These results strongly suggest that high distance variability, after prolonged practice, is a psychotic feature. At the third trial block of Experiment II (trials 17-20) differences

TABLE IV

Practice Scores of Distance Variability, Computed Between the Two Last Trial Blocks, Differentiated Significantly Between Hysterics and Psychotics (mainly Schizophrenics)

Distance Variability		Hysterics	Psychotics (I) Schizophrenics (II)	t-ratio
Level Score (2 block of trials)				
Experiment I	$\bar{X}$	24.231	20.429	1.811
(trials 5-8)	SD	51.739	44.619	(65 DF, N.S.)
Experiment II	$\bar{X}$	21.067	19.000	0.197
(trials 9-12)	SD	32.817	26.834	(28 DF, N.S.)
Practice Score (2-3 block of trials)		Hysterics	Psychotics (I) Schizophrenics (II)	t-ratio
Level Score				
Experiment I	$\bar{X}$	3.744	-0.464	2.736*
(trials 5-8 to 9-12)	SD	44.016	25.395	(65 DF, 1%)
Experiment II	$\bar{X}$	6.733	0.733	2.401†
(trials 9-12 to 17-20)	SD	27.293	23.810	(28 DF, 2%)

*=one-tail test, †=two-tail test (just short of 1 per cent. level).

between the three groups—schizophrenics, hysterics and dysthymics/controls combined—were significant at the 0.1 per cent. level, as assessed by an analysis of variance (DF=2/64), the F-ratio being 8.417.

4. Correlation Analysis

Table V contains a number of correlations which were computed both in the interest of the present analysis and for completeness of knowledge of this aspect of the FRT. Correlations were computed separately for four groups, three of them derived from I for which trial blocks 1-4 and 9-12 were used. As to II, where twenty trials had been used, coefficients based on all 67 subjects are quoted additionally for the final block of trials 17-20. In the case of all error measures correlations were computed separately for trial blocks. Thus, rotation error of trials 1-4 was correlated with distance variability of trials 1-4, rotation error of trials 9-12 with distance variability of trials 9-12, and so forth. For the remaining FRT scores, distance, size and size variability, which previously had not revealed systematic practice effects, a total score for all trials was used for correlation with the various practice stages of distance variability. "Size" is the actual size of the reproduced test shapes measured in mms. "Size variability" was computed as "between trials" plus "between shapes" variability between successive trials. These expressive movement scores have been described elsewhere (6). Other scores used earlier were excluded from the present analysis on account of their unreliability and lack of demonstrated validity. Correlations are now shown in Table V.

An FRT (learning) task of the present high level of difficulty and test length produces a practically linear increase in variance of error scores with practice. The intercorrelation of such scores is known to increase accordingly. If distance variability is to qualify as an error variable, its correlation with the three "pure" error scores should also increase with practice, which is in fact fully confirmed.

The next result, referring to score complexity, deals with trial block 9-12

TABLE V

Correlations of Distance Variability, a Complex Movement/Error Score, with Pure Movement and Error Scores, as well as I.Q. and Age

Distance Variability		Trials 1-4	Trials 9-12	Trials 17-20
Rotation error	1	0.34†	0.56†	—
	2	0.13	0.15	—
	3	0.02	0.61†	—
	4	0.11	0.35†	0.52†
Rotation variability ..	1	0.25*	0.64†	—
	2	0.40*	0.46*	—
	3	0.31	0.55†	—
	4	0.19	0.57†	0.70†
Distance error	1	0.13	0.34†	—
	2	0.38*	0.26	—
	3	-0.15	0.32*	—
	4	-0.14	0.32†	0.53†
Distance	1	0.42†	0.59†	—
	2	0.53†	0.73†	—
	3	0.70†	0.32*	—
	4	0.67†	0.47†	0.37†
Size	1	0.19	0.23*	—
	2	0.42*	0.53†	—
	3	-0.14	-0.18	—
	4	0.22	0.07	0.09
Size variability	1	0.23*	0.29†	—
	2	0.53†	0.62†	—
	3	-0.17	-0.13	—
	4	0.20	0.23	0.26*
I.Q.	1	-0.16	-0.39†	—
	2	-0.01	-0.11	—
	3	-0.24	-0.07	—
	4	0.02	-0.15	-0.32†
Age	1	0.01	-0.09	—
	2	0.17	0.26	—
	3	-0.08	0.02	—
	4	-0.10	0.02	0.10

*=5 per cent. level; †=1 per cent.

1=Exp. I: 100 neurotics.

2=Exp. I: 28 psychotics.

3=Exp. I: 40 controls.

4=Exp. II: 67 normals and abnormals.

only and is discussed jointly with results drawn from two previous publications on the same experiments and samples (3, 6). The expectation is that correlations within a group of relatively pure error scores and within a group of relatively pure expressive movement scores should be highest. Correlations between pure error and pure expressive movement scores should be lowest. Correlations between the movement contaminated error score (distance variability), on the one hand, and pure error and expressive movement scores on the other, should, due to the complexity of distance variability, assume an intermediate position. This expectation is fully borne out as the following mean correlation shows (means of several normal, neurotic and psychotic groups).

Mean intercorrelation of error scores (rotation, rotation variability and distance error), uncontaminated by expressive movement (3)	0.70
Mean intercorrelation of expressive movement scores (distance, size, size variability), uncontaminated by error (6)	0.61
Distance variability vs. three error scores, uncontaminated by expressive movement (Table V)	0.43
Distance variability vs. three expressive movement scores, uncontaminated by error (Table V)	0.31
Movement uncontaminated error scores vs. error uncontaminated expressive movement scores (6)	0.10

Correlations of distance variability with intelligence are somewhat higher than those for expressive movement measures *per se*, which may be explained as due to the error content of distance variability.

DISCUSSION

It has been demonstrated that the use of a complex error score, involving components of expressive movement in addition to error, may lead to contradictory results in terms of relationships to personality criteria. If learning scores proper, i.e. scores not contaminated by irrelevant factors like expressive movement, are used, the consistent result apparently is that extraversion is not significantly related to learning or, under certain conditions, that extraverts perform superior to introverts. Inconsistencies introduced by recent motor pursuit experiments, reviewed by Jones (10), have thus been resolved, and the models proposed to explain the discrepancies may now be considered redundant. We readily agree with Jones, however, that the earlier conceptions of learning as related to extraversion require considerable change. Eysenck (7) expected practice effects to be lower in extraverts due to their stronger "inhibitory potential" and to be higher in introverts due to their stronger "excitatory potential". After the present analysis of score complexity, however, either the opposite is true, or the inhibitory phenomena under discussion are not significantly related to extraversion, or extraversion plays a part under certain restricted conditions which are impossible or difficult to predict on a purely rational basis. The present author favours the latter explanation.

If extraversion is to be further investigated in relationship to learning, the indications are now that control of movement characteristics of the kind described is a *conditio sine qua non* and that significant sources for the measurement of extraversion may be derived from a systematic variation of stimulus and task conditions. It has already been shown that level of difficulty and/or exposure time are of importance in this respect (4). A carefully manipulated condition analysis would then lead to the construction of an empirically rather than conceptually derived learning model which, to be of any great use, must include other factors already known to, or claiming to, play an important part (4). Rigidity, for example, has already been demonstrated to be significantly related to learning and Taylor's Manifest Anxiety Scale, despite its somewhat high correlation with Eysenck's dimension of neuroticism, may be expected to play a specific role.

Of the various results one point is particularly outstanding. Differences in distance variability, a complex movement/error score, were by far greatest

during early practice and disappeared with prolonged practice. This is quite typical of pure learning scores, derived from the same test and the same subjects, where individual and group differences are smallest in the beginning and increase with practice. The most obvious interpretation of this discrepancy is that differences found in early practice stages are due to differences in the movement component rather than error. This explanation appears equally applicable to pursuit-rotor and related performances, where differences between extraverts and introverts were largest in the early stages and practically absent during later stages of practice (2, 7, 9, 10). This apparent learning paradox is then satisfactorily resolved by using expressive movement as a basis for interpretation rather than learning proper.

The point just made permits one further generalization. If, in complex movement/error scores, the movement component is of greatest importance it is obvious that reminiscence will be affected similarly to level or practice scores. One would expect then that reminiscence scores obtained during early practice stages correlate higher with extraversion than those obtained during later stages. This was in fact found to be the case (7, 9, 10). As this point is elaborated in a separate study we confine ourselves at the present to the general suggestion that, in practice and reminiscence scores of motor learning alike, results relating to extraversion are easily explained in terms of expressive movement but difficult to understand in terms of learning proper.

The results obtained with the psychotic groups, which consisted mainly of schizophrenics, are difficult to explain. Although highly variable with respect to the actual size of test shapes reproduced psychotics were, in early practice stages, not particularly variable on distance scores, regardless whether error was involved (Fig. 2) or not (6). Although on pure learning scores psychotics scored significantly highest error at trial block 9-12 (3), this was not the case for distance variability. It has, however, been shown that distance variability of the psychotics tends to become highest with prolonged practice (trial block 17-20), regardless of the inclusion (Fig. 2) or exclusion (6) of error from distance variability. This may suggest that the movement component is of greater importance. Results certainly are in agreement with the long known observation that schizophrenics lack ability to maintain a level of mental preparedness over a long time. This inefficiency appears to be of a highly general kind, as it has been demonstrated with regard to reaction time, pursuit learning, learning of visual patterns, exposure time in recall and movement variability (compare 4).

SUMMARY

A learning test requiring drawn reproductions of visually presented patterns was analysed for two types of error scores, either contaminated or not contaminated by characteristics of expressive movement. When related to hysteria/dysthymia, the abnormal variants of Eysenck's dimension of extraversion/introversion, hysterics scored inferior to dysthymics on a score contaminated by movement extent and variability. When movement effects were controlled, opposite results were obtained, i.e. hysterics tended to score superior. This finding was used to question the validity of a number of pursuit-rotor experiments in their relationship to extraversion, on the ground that the inferior performance of extraverts found is solely due to their greater movement extent and variability. In contrast to this, learning efficiency proper is shown to be higher in extraverts than in introverts, or not related to extraversion at all. A specific effect dealing with prolonged practice was demonstrated in a schizophrenic group.

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PSYCHOMETRIC TESTING IN IRAN

By

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GENERAL BACKGROUND OF THE IRANIAN CULTURE

GEOGRAPHICALLY, Iran is for the most part a plateau at an average altitude of about a thousand metres; the terrain—the near-jungle conditions of the Caspian littoral excepted—is arid semi-desert and the typical landscape one of limestone mountain ranges between which lie flat plains. It is bordered by Iraq, Turkey, the U.S.S.R., the Caspian Sea, Afghanistan, Pakistan and the Persian Gulf. Except during spring the vegetation in most areas is sun-shrivelled on the bare treeless earth; after five months of burning heat the winter rainfall washes off the topsoil, and river-beds, dry throughout the summer, overflow with muddy torrents. Although the soil is dry and powdery there are abundant water-courses below the surface and with irrigation the land is very productive. The country has great mineral resources; apart from the oil-fields they are unexploited due to lack of coal and road and rail communications. The people are mainly feudal villagers or nomadic tribesfolk but Tehran is a modern city of a million population and the provincial capitals are also semi-Westernized.

Persian history is said to extend over some six thousand years and reached its apogee two and a half millennia ago when Cyrus the Persian, having conquered the fabled wealth of Croesus of Lydia, went on to assimilate the Babylonian empire of Belshazzar: this occurred after the renowned feast during which a mystic hand appeared and wrote words of doom upon the wall. The couriers of his successor, Darius I, carried the royal decrees from Persepolis throughout the largest empire the world had yet seen, spreading from the Libyan desert to the Indus. Trying to extend his dominion to Europe, Darius was defeated at Marathon; his son Xerxes mounted a second gigantic expedition but a fateful end awaited it and indeed it may be no exaggeration to say that on this foray hinged the future course of history in Europe and Asia. Weakened by the struggle at Thermopylae against the heroic resistance of the Spartan Leonidas, Xerxes marched on Athens and succeeded in destroying the city, but the following year his armies retreated after defeat at Platea. Athens was rebuilt under Pericles, and the "Athenian Age", founded on free, exact and systematic thinking, began to flourish: its ideals of freedom, beauty and truth, its drama, science, architecture and philosophy, have influenced all subsequent Western culture. In turn, Persia succumbed to the Hellenic invasion under Alexander the Great, but when his empire disintegrated she again emerged as a separate state under Parthian and native Persian dynasties.

Persia then became involved in wars of attrition with the Roman and later the Byzantine Empire before succumbing to the growth of Arabic Islamic power in the seventh century, and again to the Tartar hordes of Jenghis Khan in the thirteenth. Persians often explain their present-day cultural and economic poverty, and their difficulties in getting out of it, in terms of having been a twice-conquered people. Incidentally, Iranian history as it is generally known

in Iran is a mixture of fact and myth: Darius and Alexander are almost unknown, and Persepolis is called "The Seat of Jamshyd" on the grounds that it was so magnificent that only this legendary hero could have built it.

Culturally, there is not a very strong literary representation apart from the many poets, nor has Iran much to show in the graphic or plastic arts; science, technology and industry are likewise very weak. The Persian language (Farsi) possesses a very limited range and without its many Arabic and European importations any exactitude of expression would be impossible. The Shia Mohamadism of Iran is a less strict type of religious observance than the orthodox Sunna, and does not lend itself to fanatical practices unless during the mourning days; in addition there is a proportion of Jewish people, of Bahai adherents (a schismatic sect established about a hundred years ago), of Zoroastrians (followers of the pre-Islamic religion of Persia), and of Armenian Christians.

Iranian culture, one might say, is primarily a civilization of manners. Formal types of address regulate all social intercourse and there tends to be a pyramidal system whereby one abases oneself and shows respect to superiors: many examples of this occur in polite speech which requires the liberal use of submissives and honorifics, as for instance the use of the term *bandeh* (bound one, or slave) in place of the first personal pronoun; the use of the verb *farmudan* (to command) e.g. "What do you command?" to a superior instead of "What did you say?" and the use of the second personal singular to children or servants, the plural for social equivalents or superiors. The usual greeting *Salam* (Peace) is indicative of the non-aggressive attitude which prevails, and the employment of physical violence is strongly deprecated. The traditional Oriental virtues of respect and acceptance are very prominent; no less evident is a general insecurity which may also be traditional, and derive from the previous tensions between the *khans* (chieftains), who had little reason to trust each other. Incidentally, it may be noted that social anthropologists have noted a strong need for dependent relationships towards authority-figures in the Persian culture.

INTELLIGENCE TESTING

The use of standardized tests in the West has reached such a high level of technical proficiency that it is now a matter of routine, but if we apply these methods in an undeveloped country we interfere with the basic theory of testing to such an extent that we must pause and enquire what we are really doing—what are we measuring and how are we trying to measure it?

First, as to the nature of intelligence itself, I have elsewhere put forward the view (Valentine, 1955) that the idea of "intelligence" as an abstract entity is not a rewarding concept, and that when we speak of intelligence we should recognize that we usually mean "intelligent behaviour". Now, the kind of intelligent behaviour necessary for survival as a primitive tribesman may be something quite different from the kind which succeeds in a Western metropolis; but as Western science, logic and philosophy have penetrated so deeply into the nature of the cosmos, and shown themselves to be so universally adaptable, we may perhaps be forgiven the presumption that they represent a normative ideal.

The measurement of this kind of intelligence depends variously on the perception and analysis of visual data, on linguistic ability and general knowledge, and on the employment of reasoning processes. There is evidence how-

ver that all these abilities are influenced by learning which we acquire unconsciously from prolonged exposure to training stimuli, and that they are not inborn, ready-to-use mechanisms. For example, to show that Western-type perceptual sophistication is not a feature of an undeveloped culture we have only to think of the Arabs in T. E. Lawrence's *Seven Pillars of Wisdom*, who were totally unable to recognize paintings of themselves; one perplexed tribesman even suggested that a portrait might be a sketch of a camel as the curve of the jaw reminded him of the animal's hump. There are obvious difficulties therefore in transferring Western perceptual tests to other cultures; and since these visual, "non-verbal" tests based on perceptual reasoning were designed for the express purpose of avoiding the complications of types based on language and general knowledge, it seems scarcely necessary to stress the point that linguistic tests cannot always be directly translated. In the third place, it is easily demonstrated that many aspects of reasoning as known in the West are also learned processes, because much of the thinking in undeveloped societies is of a prelogical type, unrelated to causality and showing only a crude appreciation of number. This is normal in the West as a phase in child development, but it does not usually persist after the age of about eight years, when analogical reasoning begins to be used; we also find such thought processes in old people and in some psychiatric cases. Bromley (1953) has pointed out that the kind of observation and clear thinking, or intelligence, demanded by a test such as the Progressive Matrices (Raven, 1938) is a highly developed form of abstract, objective, logical, relational thinking, whilst the intelligence of primitive groups is of a qualitatively different kind—a lower order of concrete, subjective, alogical, global thinking.

These points should be sufficient indication that no test is so free from cultural influence as to permit it to be used unquestioningly in alien societies.

Intelligence Testing for University Entrance

The Progressive Matrices (1938), a convenient non-verbal group test, was used for the selection of candidates for higher education in Shiraz and the results are demonstrated in Figure 1. The test was given a rather more elaborate

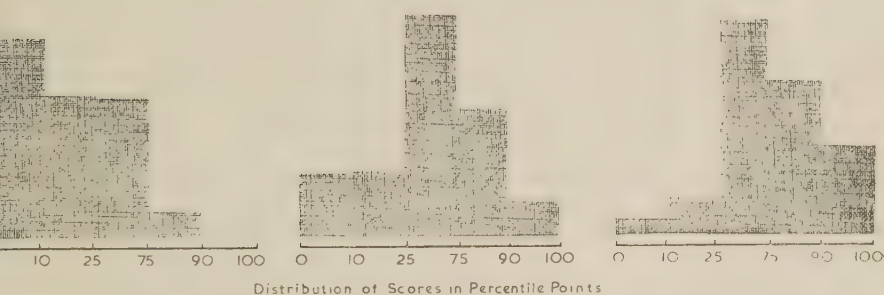


FIG. 1.—Distribution of Intelligence Test scores in candidates for higher education.

introductory description in Farsi (Persian language) than is specified for the English-speaking procedure, and the score-sheets were printed in Farsi. Three groups were concerned; first, candidates for the *Agricultural School*—young men from villages and small towns applying for a 1- or 2-year course in farming methods, mainly practical but with some theoretical instruction. The test procedure was completely unfamiliar to them; they had evident difficulty in

assimilating and handling the data and it will be seen that the scores are very depressed by Western norms, probably due to a combination of novelty and anxiety. The majority return "subnormal" scores and almost one-third are in the "defective" category (0-5 percentile points). It would however be ridiculous to translate these results literally in terms of Western standards.

The second group consisted of candidates for admission to the *Medical Faculty* and an intensive selection procedure was followed as there were 340 applicants for 60 places. This number was reduced to 120 by (a) a test of academic achievement in basic sciences and (b) a test of linguistic ability; then the 120 were given (c) the Matrices test and (d) the Ranking Rorschach test. As the middle section of Figure 1 indicates, a much more normal distribution occurred here. There are however two discrepancies: first, the results are only "average" whereas this is in fact a selected group; and second, there should not be in a selected group a "tail" of poor performers such as is shown by the 12 per cent. scoring less than 10 percentile points.

The third test was carried out on applicants for the *Agricultural Faculty*. Here the results much more nearly resemble those expected from a similar Western group, with a definite bias towards the higher percentile ratings. One presumes the reason to be that this group was tested last, and candidates had acquired some idea of what to expect. Also, the group contained some candidates who, having been rejected at the medical entrance examination, made a try for admission to the agricultural college; with recent experience of the test they might be expected to do better at a second trial. However, an anomaly persists in the distribution in so far as a few students seem quite unable to organize the material, and return unduly low scores.

As regards the success of the selection procedures, results are available at present only for the first year. However, even in the first few months of the sophomore year the agricultural and pre-clinical teachers were enthusiastic about the selection method as the classes showed exceptionally high average grades in examinations and the students were noticeably keen and capable.

It is clear that the proper selection of students is absolutely vital to the success of any such new educational establishment, for otherwise candidates may be admitted having a severe and practically irremediable limitation of logical thinking which will affect their potential level of attainment. Also it is important that during the first year of higher education, students should be exposed to a basic orienting course providing training in logical thinking and methodology: this need not necessarily be a separate course, but may be learned indirectly from the curriculum subjects provided they are sufficiently well taught. Reasons for this may be adduced from the present study: it will be seen that the younger students are more hopeful, constructive and independent in outlook and if at this stage they are provided with adequate tools for thought they will be able to bring a critical scientific attitude to the remainder of their education. Otherwise—and especially so in unselected student groups—their level of achievement is very likely to be markedly attenuated.

Intelligence Testing in the Schools

Formal education in Iran begins at the age of 7 years and proceeds through 6 years of primary and 6 years of secondary instruction in sex-segregated schools: the "certificate" is thus taken at the age of 19, or possibly later. Teachers are not fully employed and generally work 20+ hours per week; in primary schools they have no higher qualifications.

Over 600 Shiraz secondary school children were tested with the Progressive Matrices (1938); as the British norms show little variation between 14–20 years, pupils of this age group were tested. They were drawn from 4 secondary schools, 1 for boys and 2 for girls; from the results a cumulative percentile ogive was constructed (see Fig. 2); the curves have not been smoothed and the British norm is added for comparison.

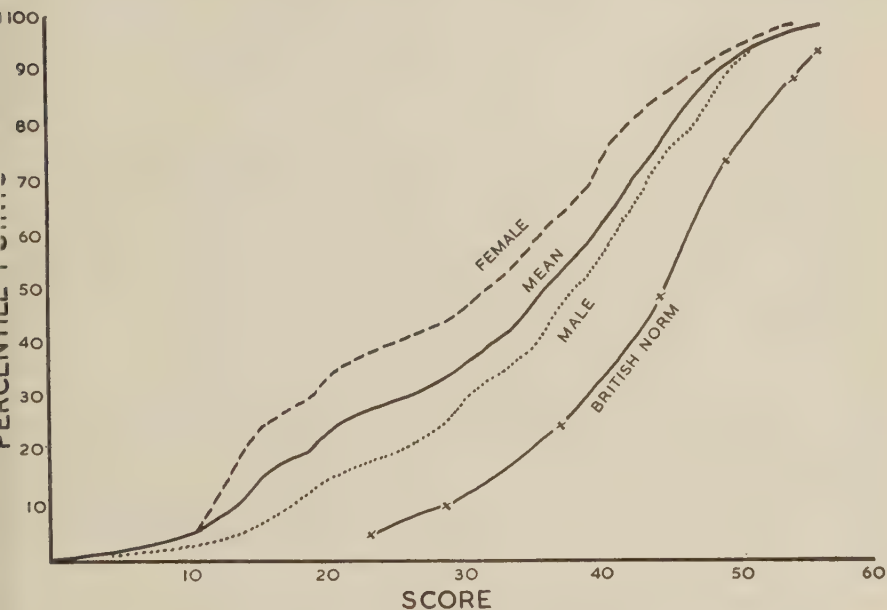


FIG. 2.—Progressive Matrices (1938). Cumulative percentile ogive showing the results of intelligence testing in 627 Shiraz secondary school pupils.

— Mean
 Male
 - - - Female

× — — × British norm (for comparison)

The Iranian scores are notably lower than the Western norm, specially towards the low end of the curve (below score 20), where the discrepancy becomes acute. Raven (1958) notes that this feature of the test score distribution has occurred in African and Oriental populations, and he explains the unduly low scores as likely to be the product of non-Westernized members of the community who have not learned to think analytically—an observation with which my own views are in accord.

Common Errors. To find out what kind of mistakes were commonly made, and to throw light on the thinking processes, a sample of 50 records was analysed. These were from subjects of 14–20 years, equal numbers of male and female, and the sample was randomly selected from subjects scoring approximately 35, as higher scores contain too few errors and lower scores are likely to be an indication that much of the test was imperfectly understood; also, 35 was in any case approximately the median score. The number of errors made on each problem is shown in Figure 3. One point which jumps to the eye is that although Set A shows by far the smallest number of errors, and Set E the largest, Sets B, C and D appear to be of almost equal difficulty.

Some common errors are selected for particular attention. The criterion for a "common error" was that it should have been selected in more than 25 per cent. of all wrong choices.

Set		Problem												Total Errors for Set
		1	2	3	4	5	6	7	8	9	10	11	12	
A	No. of errors . . .	0	0	1	1	1	0	11	6	4	10	12	28	74
	Commonest mistake	—	—	—	—	—	—	4	—	—	6	5	6	
B	No. of errors . . .	3	5	5	12	20	19	22	42	40	37	44	40	289
	Commonest mistake	—	—	6	4	5	4	2	5	1	1	3	2	
C	No. of errors . . .	7	16	9	18	19	26	19	26	19	40	35	46	280
	Commonest mistake	3	3	—	3/4	4	—	7	—	—	8	—	3	
D	No. of errors . . .	9	14	14	19	12	22	23	30	25	40	39	47	294
	Commonest mistake	4	3	4	2	—	3	—	3	—	5	—	7	
E	No. of errors . . .	19	32	28	46	40	43	41	48	45	45	49	49	485
	Commonest mistake	4	3	2	3	7	8	6	8	—	7	1	—	

FIG. 3.—*Progressive Matrices* (1938). Common errors for each problem of Sets A, B, C, D and E in a random sample of 50 subjects.

In Set A the first stumbling-block is problem A7—perhaps because this is the first pattern which requires the formation and completion of a gestalt; a similar but doubled element is the popular mistake. In A10 a smaller and laterally inverted pattern is selected, and in A12 a smaller but correctly oriented example.

Set B presents no great difficulty until B5, an asymmetrical pattern; here the fault is to repeat a portion of the pattern. Problems B8 to B12 mark the commencement of analogical reasoning and in all of these the common error is again to repeat a part of the given pattern. In general terms, these problems ask

$$\begin{array}{l} \text{if } a \rightarrow a_1 \\ \text{therefore } b \rightarrow ? \end{array}$$

The usual mistake is to reply by repeating the fundament " a_1 " instead of searching for the correlate which would give a " b_1 " category of solution. This may be regarded as perseveration of a method which was learned in the early part of the series—that is, pattern-completion by repetition of a discrete part. The facts that the pattern is now asymmetrical, and that it needs analogical deduction for its solution, are equally ignored.

Frequent errors in Set C are:

- C2 Use of symmetrical instead of progressive completion.
- C4 Failure to complete both the vertical and horizontal progressions.
- C5 Use of balanced symmetrical completion in place of progressive completion.
- C7 Failure of vertical balanced symmetrical completion.
- C10 Failure to separate the elements.
- C12 Failure of horizontal completion.

In Set D we find:

- D4 Repetition of a discrete part.
 - D6 Selection of "figure" but not of "ground".
 - D8 Selection of configuration but not of its patterning.
 - D10 Inclusion of an unnecessary element.
 - D12 Failure to appreciate both the numerical progression and the change in the patterning;
- and in Set E

- E1, 2 and 3 Omission of an element from the superimposition.
- E4, 5, 6 and 8 Attempted symmetrical completion instead of subtraction of elements.
- E7 The lower element is altered instead of being retained.
- E10 and 11 Failure to appreciate the subtraction of elements.

It may be questioned whether any errors are due to the use of the test in a different culture from causes such as the following—the pages of the test book turn from right to left instead of in the Persian fashion of left to right; the examples run from left to right, and the patterns have to be completed from left to right, whereas the Persian student reads from right to left.

From observation it is believed that neither of these factors is important in the groups tested, as the subjects were familiar with English books. Once the "set" of the problems has been acquired, the lateral inversion of the subjects' normal habit of scanning from right to left loses importance.

PERSONALITY TESTING

Personality tests were used both in selection procedures and as a method of investigating personality structure in Iran.

Ranking Rorschach Test

The multiple-choice version (Harrower-Erickson and Steiner, 1945) was employed as part of the selection procedure. There are of course arguments for and against the use of such tests in populations for which they have not been standardized but on balance it seems a justifiable and useful thing to do. It has been amply demonstrated that academic failure is related to emotional instability rather than to lack of intelligence and so any measurement of emotional stability, however imperfect, is not to be discarded lightly. Also, whatever one may believe about the value of the determinants, and the content of the responses, the test shows up candidates with a low number of responses and this inhibited productivity is in itself one of the most valuable indications. The multiple-choice Rorschach possesses another advantage not shared by all group projection tests—it can be scored by unskilled clerical staff. In the testing, standard group procedure was followed; the score-sheets had been translated into Farsi.

The graphs of the entry for the agricultural and medical faculties are shown in Figure 4. The distribution, and the median scores, seem to resemble Western

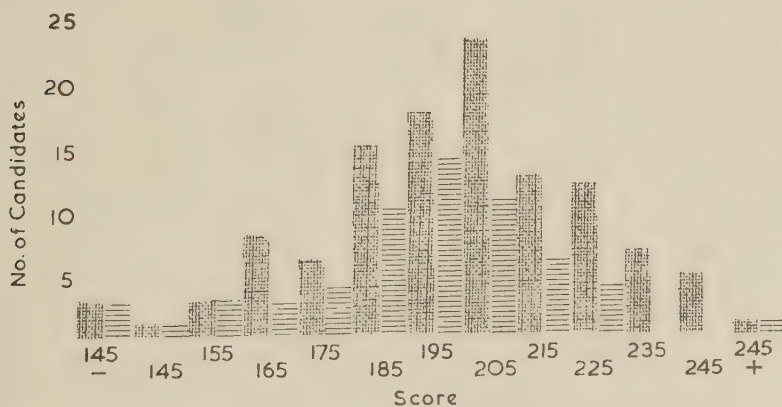


FIG. 4.—*The Ranking Rorschach Test*. Dotted bar graph—115 candidates for entry to Medical Faculty, Shiraz University, 1957. Median score=203.
Lined bar graph—60 candidates for entry to Agriculture Faculty, Shiraz University, 1957. Median score=194.

norms sufficiently to enable one to pay some attention to the results. It will be noted that the median scores (194 and 203) are lower than Eysenck's (1947a) findings, which were generally about 230. This could be interpreted as a tendency on the part of the students to show more neuroticism than corresponding Western groups.

"Study of Values" Test

Another test relevant to selection purposes—although not used as such in the present instance—is that devised by Vernon and Allport (1931). This is a questionnaire type of test and the scores allotted by the subjects are used to build up a profile of his interests from six aspects—theoretical, economic, aesthetic, social, political and religious. The mean standard score is 40 with an average range extending from 30–50. The results in a group of 37 medical freshmen were as follows:

Theoretical	..	..	..	..	46·9
Social	..	..	..	..	43·9
Political	..	..	..	..	41·9
Aesthetic	..	..	..	..	37·3
Religious	..	..	..	..	35·1
Economic	..	..	..	..	34·5

The theoretical interest is supported by a strong tradition of polemic and discursive argument, and the marked social trend we shall encounter again in considering the findings of other tests. There was a remarkable uniformity in the results, and deviations from the mean were very small.

The Maudsley Medical Questionnaire

This test (MMQ) was developed in Britain as an index of neuroticism; it consists of 40 questions containing common neurotic symptoms, and the score is based upon how many positive answers are returned. In common with all such tests it has the defect of being dependent on "what the subject says" but nevertheless is of value in giving the subject's own estimation of himself.

The mean score of 147 medical students was 11·0, a figure slightly higher than the published norm (Eysenck, 1952) of 10·0 for an average population. However, considering that the personnel in the present experiment were not an average but a selected group, young medical students in good health and of better-than-average intelligence, the score is high, as Eysenck (1947b) found a mean of only $2·81 \pm 2·60$ in his supernormal group of officers. On breakdown there were no significant differences between students of different years, the range being only from 9·7 to 12·8 and the means of the junior students (1st, 2nd and 3rd years) not differing significantly from those of the senior students (4th, 5th and 6th years). The mean for women students was slightly but non-significantly higher at 12·9.

The questions which were answered most often in the abnormal direction were—worry over humiliating experiences, feelings of inferiority, feelings easily hurt, lack of vitality, and self-consciousness with superiors. Questions answered least frequently in the abnormal direction were—nervous breakdown, liability to diarrhoea, dizzy turns, severe headache, and traumatic unconsciousness.

The women students returned the greatest number of abnormal replies on the following—worry over humiliating experiences, off work through sickness, feelings easily hurt, touchiness, and aches and pains; possibly dysmenorrhoea may account for Questions 4 and 36 (off work through sickness, and aches and pains). It is interesting to note that concern over humiliation and sensitiveness are again dominating themes; also that these results are not in accordance with the British rank order of questions having diagnostic value for females, except in so far as Question 4 (off work through sickness) is second in rank order both in the British results and in the present investigation. "Worry over humiliating experiences" and "feelings easily hurt" were questions

found by Eysenck to have a higher diagnostic value for men, but here they rank respectively first and third in each sex. The questions which the women students answered least frequently in the abnormal direction were—dizzy turns, palpitation, traumatic unconsciousness, sleepwalking, sweating, and liability to diarrhoea.

Thus both sexes tend to reject the questions indicating a liability to anxiety or hysterical symptoms, the women more strongly so than the men. Also, lack of self-confidence tended to be denied in spite of admission of inferiority feelings, and also self-consciousness in the presence of superiors.

The general pattern of the results is of interest because although the mean score tends towards the "neurotic" pole, the score is composed of elements other than we would expect in an English-speaking sphere. There are no hypochondriacal complaints, no irritability or feelings of loss of concentration, nor any anxiety, depressive or compulsive features. The subjects do not appear to be "somatic reactors" and the adjustment difficulties appear to lie directly in the field of relationships to power-figures, with widespread concern over humiliation, inferiority and sensitiveness. What this may mean in terms of dynamic structure, or social culture, is not revealed by the present study; but there are indications that the personality structure of the students is, by Western standards, under some stress. A questionnaire designed to allow expanded and explanatory answers was next given out, but the response to this was poor and such replies as were received did not throw much light on the reasons for the prevalent feelings of humiliation and inferiority.

Character Trait Rating Scales

The population in this experiment consisted of over 100 medical students at the University of Shiraz; their median age was 24 years, the range extending from 19–35 years. By religious persuasion, the largest proportion was Moslem (65 per cent.), the next biggest group being Bahai (8 per cent.); Zoroastrians, Jews and an atheist constituted 9 per cent., and for one reason or another no reply was returned by 18 per cent. Female students amounted to 10 per cent. of the whole group.

The character trait rating scales evolved by Goldman-Eisler (1953) were used; these consist of questionnaires on which the subject ranks his score on a scale -2 , -1 , 0 , $+1$, $+2$, attesting his opinion on first, a group of proverbs and aphorisms, and second, an analytic questionnaire modified from H. A. Murray's (1938) scales. In all there are 150 questions, giving ratings on 19 character traits; these follow, accompanied by brief working definitions:

Optimism—a hopeful attitude without reasonable expectation, psycho-analytically stated to derive from persistence of an infantile sense of magical omnipotence. *Pessimism*—inability to accept frustrating experiences as part of reality, and an anticipation of disappointment as a defence mechanism. *Passivity*—an indolent, receptive attitude. *Desire for the Unattainable*—ambition combined with a sense of the impossibility of achievement. *Displaced Oral Aggression (Verbal)*—aggressive use of speech. *Aggression*—desire to attack and overcome opposition. *Aloofness*—a negative and rejecting attitude to people. *Ambition*—striving to overcome and excel. *Autonomy*—independence of attitude. *Dependence*—helplessness and desire for love and protection. *Guilt*—self-torture from retributive images. *Change*—inconsistency and lack of fixed attachments. *Conservatism*—enduring sentiments and loyalties, rigidity of habits. *Impulsiveness*—acting without

reflection. *Deliberation*—hesitant, inhibited action. *Exocathexis*—participation in practical and co-operative action. *Endocathexis*—preoccupation with inner life. *Nurturance*—tendency to aid and protect. *Sociability*—ease in personal relationships, tendency to join groups.

The results were sorted into four sub-groups—youngest and oldest male Moslem students, female students, and Bahai students, and the results are set out in tabular form in Figure 5, and graphically in Figure 6. No standard deviations are available so conclusions cannot safely be drawn from small differences.

From general consideration of the table it may be seen that the group as a whole is fairly homogeneous and that there are no abrupt reversals of sign for any of the traits in different sub-groups. Also the paired opposites such as optimism-pessimism, change-conservatism, deliberation-impulsiveness, sociability-alooftness and exocathexis-endocathexis show the expected polarity. However, in interpreting this sort of test the results cannot be taken at face value; subjects may answer, not exactly what they feel, but what they think they ought to feel. Hence the results may indicate identification with some desired culture pattern rather than the existing reality.

First considering the differences between the sub-groups, it is noted that the younger male students (average age 21 years) show less pessimism and a stronger denial of passivity than the other Moslem groups, also they are less aloof and more sociable than the other male groups. Oral aggression is refuted, dependence is fairly strongly denied and endocathexis (roughly corresponding with introversion) is feeble.

The older students (average age 31 years) show the highest pessimism score, and desire for the attainable is denied—possibly an indication of increased reality thinking. Passivity and aloofness are less strongly rejected, and this is accompanied by a feeling of possessing less individual autonomy; the deliberate, introverted outlook has increased. These findings suggest that the older students are more reflective and less hopeful of achievement through individual effort; whether they grew up with this attitude, or whether they acquired it in becoming mature, cannot at present be determined.

The female student group is notable in showing low optimism and high pessimism scores. Although the ambition score is the highest for all sub-groups, passivity is not strongly rejected. Desire for the unattainable is less strongly rejected than by other groups and as might be expected, the nurturance score is the highest.

Bahai students have the highest optimism score and are also the only group to return a minus score for pessimism; passivity is also refuted more than by other groups but despite this the ambition score is the lowest. This group is the only one to show a moderate positive score for guilt feelings; sociability is slightly low. Although endocathexis is moderately high, with an associated lowering of the exocathexis score, it is perhaps surprising to find deliberation lower. The group appears to be more resistant to change, and more conservative, than others.

So much for the sub-groups. From a general review of the table, it is evident that 9 traits make up most of the scoring. Taking the group averages, there is a positive swing towards (in rank order), sociability, exocathexis, nurturance, optimism, ambition and deliberation. The negative scores, weaker and all of about the same strength, indicate a denial of displaced oral aggression, of desire for the unattainable, and of passivity.

Trait				Youngest Male Moslem Students	Oldest Male Moslem Students	Female Students	Bahai Students	Mean
Optimism	..	..	..	+1·16	+1·01	+0·88	+1·23	+1·08
Pessimism	..	..	..	+0·05	+0·21	+0·20	-0·26	+0·08
Passivity	..	..	..	-0·66	-0·32	-0·36	-0·70	-0·50
Desire for unattainable	..	..	..	-0·58	-0·71	-0·36	-0·48	-0·56
Displaced oral aggression	..	..	..	-0·76	-0·66	-0·42	-0·41	-0·51
Aggression	..	..	..	-0·27	-0·35	-0·20	-0·27	-0·28
Aloofness	..	..	..	-0·38	-0·03	-0·50	-0·16	-0·25
Ambition	..	..	..	+0·80	+1·02	+1·16	+0·76	+0·93
Autonomy	..	..	..	-0·03	-0·23	-0·17	-0·06	-0·12
Dependence	..	..	..	-0·54	-0·16	-0·32	+0·03	-0·29
Guilt	..	..	..	-0·09	+0·09	+0·09	+0·22	+0·05
Change	..	..	..	-0·14	-0·10	+0·01	-0·22	-0·11
Conservatism	..	..	..	+0·22	+0·35	+0·37	+0·42	+0·32
Impulsiveness	..	..	..	-0·15	-0·42	-0·15	-0·28	-0·25
Deliberation	..	..	..	+0·91	+0·99	+0·85	+0·61	+0·84
Exocathexis	..	..	..	+1·25	+1·24	+1·28	+0·96	+1·20
Endocathexis	..	..	..	+0·07	+0·21	-0·09	+0·22	+0·14
Nurturance	..	..	..	+1·10	+1·15	+1·27	+1·16	+1·09
Sociability	..	..	..	+1·41	+1·16	+1·27	+1·09	+1·23

FIG. 5.—*Character Trait Rating Scales*. Results for 4 student groups and the general means for each trait. Scoring is by a self-awarded point scale ranging from -2 to +2.

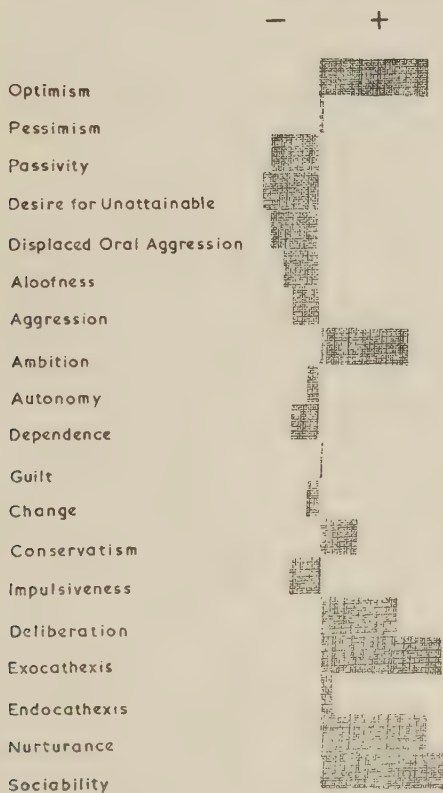


FIG. 6.—The Character Trait Rating Scales—bar graph of the mean values. (See also Fig. 5).

The results seem, generally speaking, to be in accordance with expectation. Iranian students are notably gregarious and show protective and—superficially

at least—cheerful attitudes together with a desire for improvement; Western observers might not agree with the deliberative tendency which is claimed. It would be admitted that aggression and verbal aggression are infrequent in Iran, but the denial of passivity might meet with less agreement. Also, although ambition is ranked moderately high, this fact does not run well in accord with the tendencies to conservatism, dislike of change and lack of personal autonomy. There may be some conflict over autonomy and dependence, as here both are denied whereas one would ordinarily expect them to be scored oppositely in sign. It is interesting that guilt feelings attract very little scoring.

All of the foregoing has been based on the fact that this test was standardized in England; that is to say, experiments were done to ensure that subjects possessing the trait of conservatism, for example, would in general tend to mark positively all questions in this section. However, when the test is used in a different country cultural factors may affect the issue and there is no guarantee that the questions in any section will be answered consistently. To determine this point, a random sample of the questionnaires was analysed to show how each question was answered, as a check on possible discrepancies. After each heading is given the mean score for all students.

Optimism (mean score +1.08). All statements were answered positively except for two which had weak negative scorings—"Ill luck is good for something" and "Most people can be trusted".

Pessimism (mean score +0.08). The statements in this section were answered rather variably but there was general agreement about two of them—"Hardly anyone cares much what happens to you" and "Real friends are hard to find".

Passivity (mean score -0.50). The largest contribution here came from the refutation of the statement "An idle life is the life for me, idleness spiced with philosophy". There were no major discrepancies in the answering.

Desire for the Unattainable (mean score -0.56). All statements were answered negatively except for one having a zero score.

Displaced Oral Aggression (Verbal) (mean score -0.51). Again all statements were negative except for one weakly positive—"Are you fond of arguing?" This is in keeping with the observation that there are social prohibitions on the verbal expression of anger in Iran but that argumentative discussion is not classed as a hostile manifestation.

Aggression (mean score -0.28). Replies here indicate a fairly well-marked inhibition of aggressive impulses, the subjects denying the questions "Are you apt to express your irritation rather than restrain it?" and "Do you often let yourself go when angry?"

Alloofness (mean score -0.25). Most questions were answered negatively but it was noticeable that a preference for the company of older and talented people is not regarded as a factor of aloofness.

Ambition (mean score +0.93). There were no major discrepancies in the replies under this heading.

Autonomy (mean score -0.12). This section was answered very variably but students indicated strongly that they did not "tend to disregard the rules and regulations hampering freedom".

Dependence (mean score -0.29). Feelings of being neglected, and tendencies towards discouragement, are denied. Only one question was answered positively—agreeing that it was advisable to ask people's opinion before making a decision.

Guilt (mean score +0.05). Replies were very variable here and no reliable pattern could be discerned.

Change (mean score -0.11). Again answers are rather mixed. There is a suggestion that new things are welcomed but that the emotional cathexis to people and to social institutions is stable.

Conservatism (mean score +0.32). Most scores are weakly negative but there is substantial agreement that a regular and routinized life is desirable.

Impulsiveness (mean score -0.25). The subjects say that they think and act quickly. The emotional components of impulsiveness however are repressed and this accounts for the minus score.

Deliberation (mean score +0.84). Questions are answered compatibly except that there is a claim to favour quick decisions.

Exocathexis (mean score +1.20). All questions were answered in the same (positive) direction and there is a strong identification with the idea of being a "practical" person.

Endocathexis (mean score +0.14). Scoring is mixed but there is a tendency to deny withdrawal and philosophizing.

Nurturance (mean score $+1.09$). All scores are fairly high apart from a denial of feeling much interest for children.

Sociability (mean score $+1.23$). All scores are high, with no discrepancies.

DISCUSSION

First it may be noted that when these scales were developed by Goldman-Eisler the purpose was to test a theory, put forward by anthropologists and psycho-analysts, to the effect that prolonged suckling and nurturance of the child, with late weaning, are related to the personality traits of generosity and optimism and to co-operative and peaceful behaviour. The investigation conducted by Goldman-Eisler supported this, and she found that late weaning tended to be associated with what she called the "oral optimist" personality type, characterized by an optimistic outlook (optimism positive, pessimism negative), an interest in external events (exocathexis positive, aloofness negative), a sociable, generous, peaceable and protective attitude (sociability positive, nurturance positive, aggression negative, verbal aggression negative), moderate ambition (ambition positive) and a tendency to welcome new experiences (change positive, conservatism negative). The results in Iran are largely in accord with this formulation except that pessimism tends towards zero instead of being negative, and also the sign values for change and conservatism are reversed. As breast-feeding is continued in Iran for 15-18 months, there is an interesting parallelism in the results.

Second, the breakdown helps to show up differences between Western and Iranian thinking, because if the statements about a trait are all answered in a similar direction—either positive or negative—then it must be regarded in much the same light as in the West, where the items were standardized. On this reasoning we find that the students' conception of sociability, exocathexis, ambition, aggression and passivity must be broadly similar to our own.

However, when some of the statements about a trait are answered in the same way as in the West, and others differently, the resulting score tends towards zero and it indicates that the constellation of statements does not amount to a consistent similar trait in the Iranian view. Thus we find that the students' ideas on autonomy, change, endocathexis, guilt, and pessimism, differ from our own. Individual freedom is sought while still following social custom; change is desirable but existing institutions are to be retained; the reflective life is at once admired and despised; the components of guilt, though present, do not seem to follow a recognized pattern; and lack of friendship and fellow-feeling is to be anticipated rather than deplored.

The third class is of traits which are considered in practically the same way as in the West, but with a reservation. Thus, for example, one can be optimistic although unwilling to believe that people are to be trusted, or that bad fortune can be turned to good account; ways should change but one's life should remain regular; a man should look after his family but need not be much concerned with his children's upbringing, as this is woman's work.

These anomalies assist in the clarification of some ways of thought which at first appear inexplicable to Westerners. In general, the results give an impression of Iranian students as being essentially sociable, wishing to further practical developments, and with a tendency to reject the elements of culture advocating passive reflection; either they are optimistic or wish to be regarded as such. There is some emotional inhibition perhaps due to an instilled reverence for established custom which seems to produce some conflict as to how far they, as individuals, can take any effective action.

GENERAL OBSERVATIONS

A few observations drawn from clinical experience rather than from test scores may not be out of place. First, the position of women in Islamic culture lends a special aspect to personality studies. It is only recently that women have been permitted any education, and the effect of being brought up by ignorant, culturally-isolated mothers (often united by arranged marriage at an immature age), was considered by the historian Sykes (1951) to be critical in the character-formation of the child. As in most cultures where women have little place outside the home, their influence within it is strong and young children may tend to identify themselves with the maternal figure. Hoff (1958) believes that whereas the common neuroses of the West hinge on the mother, it is the all-powerful and rather remote father-figure which tends to be the nucleus of disturbance in the Middle East.

Another point is the segregation of the sexes before marriage; there are strict prohibitions against social intercourse between young people and it is impossible for friendships to exist between the sexes. Marriage has its difficulties also, as women have come to demand such massive financial guarantees against desertion, divorce and multiple marriage, that not many young men are willing to undertake it. Prostitution exists only in a limited degree; the incidence of homosexuality can only be guessed at and cases are not referred. It will be seen therefore that there is a severe limitation of sexual opportunity, and this may possibly be related to the seemingly very high proportion of cases of autoeroticism followed later by complaints of premature ejaculation and emotiogenic impotence.

There are frustrations too in the field of work, and even in the case of trained people there are many obstacles in the way of obtaining secure and adequately-remunerated employment. Thus after a free and happy childhood the young adolescent is brought face-to-face with considerable and sometimes insoluble difficulties in realizing his drives.

It is difficult to arrive at meaningful figures concerning the incidence of psychiatric disabilities, but there are differences in the relative incidence of syndromes. Schizophrenia and manic-depressive illnesses are similar to their Western counterparts; the mental content of delusional and hallucinatory states is almost invariably occupied with deistic and prophetic fantasies.

Among the neuroses, the commonest is a type of hysteria with moderate signs of anxiety. Vague, fleeting physical complaints are the symptoms, and almost universal is the complaint of being affected by a "wind" (*ba'ad*). This indicates a pain or aberrant sensation which moves from one part of the body to another; there are "hot" and "cold" winds which are ordinarily treated by "cold" and "hot" foods respectively. The typical anxiety state (with somatic dysfunction, conditioned phobias and panics) is infrequently seen unless in people exposed to a Western way of life, and obsessive-compulsive disorders are likewise not much in evidence. This is no doubt in accordance with expectation, as in more primitive social organizations the tendency is towards the evasion of conflict (and hence the production of hysteria); whereas the more sophisticated groups recognize their conflictful situations and consequently show more anxiety.

Although the more educated people are enthusiastic about the application of modern psychiatric practice, the culture is in general not ready to accept it; the idea of psychotherapy, for example, is unacceptable because there is no background of knowledge which might lead people to believe that it might be

useful. People look for some "Western magic" to cure them, preferably by injections or electricity: hence narco-analysis will be accepted if it is presented initially as "an injection to relax you", and after inhibition has been reduced by the drug the normal psychiatric technique is very effective. It is often necessary to use this kind of harmless deception in order to get patients to embark on treatment, as otherwise a mixture of distrust and culturally-determined inhibition would prevent them. Perhaps because of these same factors, however, treatment of this kind, once started, seems to be even more effective than in Western practice.

SUMMARY

Intelligence scores, as obtained by a British test of perceptual reasoning, are lower in Iranian school and university populations than the expected norms, and particularly so in non-Westernized groups. It is believed that this is due to lack of training in perceptual analysis and logical reasoning. Traditionally there is a trend towards speculative theorizing rather than closely-reasoned hypothesizing based on accurate observation, logical deduction and experiment, and this tendency is confirmed by the results of "values" tests.

Personality testing shows a tendency to uniformity of response. Students are gregarious, unaggressive and protective and show a trait resembling "oral optimism" which elsewhere has been claimed to be related to prolonged suckling, but at adolescence these beneficent traits tend to be offset by frustrations and inhibitions due to other causes. In general there is more neuroticism than might be expected but it mainly takes the form of pre-occupation with feelings of humiliation, inferiority and sensitiveness, with adjustment difficulties in the field of relationship with power figures and authority-systems. Some recommendations on educational methods in new universities are adduced. In the general population a different type of neuroticism, with features of anxiety and hysteria, is found to be very common.

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A FOLLOW-UP STUDY OF CRIMINAL PSYCHOPATHS

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ONE of the most important questions in dealing with psychopaths is the prognosis. In 1948 two of us made a study (Stafford-Clark, Pond, Lovett-Doust, 1951) of a number of criminal psychopaths and a control group of ordinary criminals in prison by clinical and EEG methods, with the object of revealing the characteristics which most significantly distinguished psychopaths from others, and which were, therefore, most crucial in diagnosis. The psychopaths were carefully chosen in collaboration with two experienced prison medical officers. The later criminal behaviour of these psychopaths and controls is equally interesting, however, if we are to understand what the diagnosis implies (Gibbens, 1951). The third author (T.C.N.G.) has therefore been conducting a continuous follow-up, with the collaboration of the Prison Commission and Criminal Records Office. The results of the first follow-up, from 1948-1953, have been published (Gibbens, Pond and Stafford-Clark, 1955), and were reproduced in the transactions of the Royal Commission on the Law relating to Mental Illness (1957). The present paper deals with a further period of study, so that the follow-up is now from 1948-June, 1957.

A description of a man's criminal record must take account not only of the number of convictions, but also of the lengths of sentences. In the case of these psychopaths, however, the length of sentence is not necessarily closely related to the gravity of the crime, since most of them had many previous convictions, and many received sentences of corrective training (2-4 years), and preventive detention (5-14 years) for offences for which a first offender would have merited a year or so of imprisonment.

The psychopaths were selected as particularly severe cases. In 18 cases, however, the original diagnostic formulation contained an element of doubt such as "borderline psychopath", "hardly acceptable", etc. These doubtful cases have been excluded from the present results, which deal with 72 psychopaths and 59 controls—a few more than in the first follow-up because some cases of doubtful identity have been cleared up.

Table I shows the subsequent convictions of psychopaths and controls. The columns from left to right refer to none, or one, two, three or more than three convictions of any sort. The lines from top to bottom refer to whether the longest sentence was for less than one year (including fines or probation), for between one and two years, between two and three years, and four to ten years. A man whose record consisted of a fine for a drunken assault, a period on probation, eighteen months' imprisonment and seven years preventive detention would be placed on the bottom right-hand corner.

TABLE I
Seventy-two Total Psychopaths

Convictions	Nil	1	2	3	3+
Less than 1 year	7*	10	2	2	3
1-2 years	—	4†	3	1	2
2-3 years	—	1	3	3	7
4-10 years	—	5†	8	6	5

* 1 dead; 1 removed to Rampton, excluded.

† 1 later died; 2 certified insane, included.

Fifty-nine Controls

Convictions	Nil	1	2	3	3+
Less than 1 year	28	10	5	—	2
1-2 years	—	—	3	—	2
2-3 years	—	2	—	4	—
4-10 years	—	1	1	—	1

It will be seen from Table I that the psychopaths, as expected, had a far greater number of subsequent convictions than the controls. The difference in the proportion in the two groups with one or no later conviction is highly significant ($\chi^2=20.52$. $P=<.01$). The controls behaved as expected: some 64 per cent. have one (often very minor) or no conviction in the next 8 years. It is usually said that some 80 per cent. of first offenders in prison never return: the present group contains 24 offenders with between one and four convictions, so that the outcome is naturally worse. It was noticeable that in most cases which were reconvicted the original diagnosis contained an element of reserve, e.g. "normal control; but rather inadequate". Although the psychopaths had a heavy rate of reconviction the outstanding feature is that as many as 24 per cent. had only one or no conviction. Clearly, the diagnosis of psychopathic personality, even in severe cases, does not inevitably portend as hopeless a prognosis as is usually implied.

It was naturally suspected that a lack of reconvictions might have been due to such factors as the offender's death or removal to a mental hospital. A search was accordingly made at Somerset House for the death of all doubtful cases. And the Board of Control has kindly made a search for admission of any of them to a mental hospital. The result of these searches is shown in the footnote to Table I. Some may have emigrated, but reconvictions in the Dominions are available. After 8 years of follow-up there are no cases in which an offender has not had a substantial period at liberty following the original sentence. Nearly all were released in 1949 or early 1950: the shortest period of liberty is 4 years, and the shortest period for any psychopath who has not been reconvicted is 5 years. There seems little doubt that such factors as death or hospitalization do not account for the relatively high proportion without reconvictions.

The psychopaths were originally divided into those without complications (34), those with epilepsy (9) and those with head injury (29). Table II shows that there is no great difference in the subsequent conduct of these groups. The ordinary psychopaths have been most heavily sentenced, but those with head injury have the highest proportion with more than three convictions, which might indicate a less serious type of offence. Nearly half of the original convictions of the head injury cases were for violent offences, a much higher proportion than in the other groups; and half of these violent cases have subsequent convictions for violence.

TABLE II

Thirty-four Psychopaths Without Head Injury or Epilepsy

Convictions	Nil	1	2	3	3+
Less than 1 year	5	3	—	—	2
1-2 years	—	1	3	—	—
2-3 years	—	1	2	—	4
4-10 years	—	—	6	5	2

Nine Psychopaths with Epilepsy

Convictions	Nil	1	2	3	3+
Less than 1 year	1	2	1	1	—
1-2 years	—	—	—	—	—
2-3 years	—	—	1	—	—
4-10 years	—	2	—	—	1

Twenty-nine Psychopaths with Head Injury

Convictions	Nil	1	2	3	3+
Less than 1 year	1	5	1	1	1
1-2 years	—	3	—	1	2
2-3 years	—	—	—	3	3
4-10 years	—	3	2	1	2

The original classification did not discriminate aggressive and inadequate psychopaths. But the clinical description makes it possible to do this retrospectively. Table III shows that there is a fairly marked difference in the rate of reconviction of aggressive and other (predominantly inadequate) psychopaths. The aggressives had a worse prognosis from almost all points of view. Contrary to the popular conception of these types of psychopaths, the aggressives produced a rather large number of quite minor aggressive offences (e.g. minor drunken assaults), their major offences being mainly acquisitive, and the inadequates often committed major crimes.

TABLE III

Twenty-six Aggressive Psychopaths

Convictions	Nil	1	2	3	3+
Less than 1 year	1	2	1	1	2
1-2 years	—	—	—	—	2
2-3 years	—	—	—	1	2
4-10 years	—	1	5	4	4

Forty-six Inadequate Psychopaths

Convictions	Nil	1	2	3	3+
Less than 1 year	6	8	1	—	2
1-2 years	—	4	3	1	—
2-3 years	—	1	3	3	4
4-10 years	—	4	3	2	1

The Electroencephalographic (EEG) Results

These studies were of course less detailed nine years ago than they are now. Even so, Table IV shows clearly that the psychopaths had a much higher proportion of abnormal records than the controls.

TABLE IV

	Normal	Abnormal	Doubtful	No Record
Controls	38	6	6	13
Borderlines	10	7	1	—
All psychopaths	29	34	10	9

Nevertheless, there is still no indication in the present material that psychopaths with an abnormal EEG had more serious subsequent criminal records. Moreover, the few controls who had abnormal EEG did not have a worse prognosis than the rest.

In the sub-groups, however, there was a slight indication that abnormality was related to prognosis. Since on the whole abnormal EEGs are found less frequently with advancing years, one might expect that prognostic significance might depend to some extent upon age. Analysis showed in fact that although in younger psychopaths it had no prognostic significance, in psychopaths of over 25, abnormality tended to be associated with a slightly more *favourable* prognosis, as Table V shows. This is, however, rather less noticeable than in the 1953 follow-up. One might conclude that in older psychopaths an abnormal EEG at least offers some hope that improvement by maturation can still occur.

TABLE V

Psychopaths over 25. Sixteen Abnormal EEG

Convictions	Nil	1	2	3	3+
Less than 1 year	2	3	1	1	—
1-2 years	—	—	1	—	—
2-3 years	—	—	—	—	1
4-10 years	—	2	3	1	1

Psychopaths over 25. Twenty Normal EEG

Convictions	Nil	1	2	3	3+
Less than 1 year	1	1	1	—	2
1-2 years	—	2	1	—	1
2-3 years	—	—	1	—	1
4-10 years	—	2	3	2	2

Secondly, Table VI shows that in aggressive psychopaths abnormality of EEG has no prognostic significance but in inadequate psychopaths is distinctly favourable.

TABLE VI

Thirteen Inadequate Psychopaths with Abnormal EEG

Convictions	Nil	1	2	3	3+
Less than 1 year	3	4	—	—	—
1-2 years	—	—	2	—	—
2-3 years	—	—	—	1	—
4-10 years	—	1	1	1	—

Twenty-one Inadequate Psychopaths with Normal EEG

Convictions	Nil	1	2	3	3+
Less than 1 year	2	—	1	—	2
1-2 years	—	3	1	—	—
2-3 years	—	—	2	1	4
4-10 years	—	2	2	—	1

Table VI—continued

Fifteen Aggressive Psychopaths with Abnormal EEG

Convictions	Nil	1	2	3	3+
Less than 1 year	—	1	1	1	2
1-2 years	—	—	—	—	1
2-3 years	—	—	—	1	—
4-10 years	—	1	3	1	3

Nine Aggressive Psychopaths with Normal EEG

Convictions	Nil	1	2	3	3+
Less than 1 year	—	1	—	—	—
1-2 years	—	—	—	—	1
2-3 years	—	—	—	—	2
4-10 years	—	—	3	1	1

Aggressive Crimes

Follow-up has shown that an unexpectedly high proportion of these criminal psychopaths remained free of further crime. The unexpected outcome has however very largely been confined to the inadequate (or perhaps rather "non-aggressive") psychopaths. The young inadequate psychopath, if he has few previous convictions, has a reasonably favourable or at least uncertain prognosis. The aggressive psychopath however is almost invariably re-convicted. Study of their actual crimes, however, again produced unexpected results. Attempted suicide was not common; only 4 men were charged with it subsequently, but no doubt others have escaped notice. The most interesting feature was the relative rarity of aggressive offences. Twenty-six aggressive psychopaths had 105 subsequent convictions, but there were only 18 purely aggressive crimes (committed by 9 individuals), 12 of which were punished only by fining. This does not include 4 cases of indecent assault in which the element of violence is very variable. The aggressive offences of the inadequate psychopaths, though only half as frequent, were sometimes quite serious. In 8 years the inadequate and aggressive groups together only committed five aggressive crimes which were punished by a year or more of imprisonment. The aggressive crimes committed by the three groups were as follows:

1. *Aggressives*. Nine of 26 individuals committed 18 aggressive offences comprising 12 fines for wilful damage, drunken assault, etc.; 3 of 3 months imprisonment or less for assault or attempted suicide; 1 of 6 months for assaulting police; 1 of 15 months and 1 of 3 years for grievous bodily harm.

2. *Inadequates*. Seven of 46 individuals committed 9 aggressive offences comprising 3 of 2 months or less for assault; 2 of 6 months for assaulting police and attempted suicide; 2 of 12 months for assault; 1 of 4 years for manslaughter and 1 of 7 years for robbery.

3. *Controls*. Four of 59 individuals committed 17 aggressive offences comprising 13 fines for assault or wilful damage; 1 each of 14 days, 2 months and 6 months for assaulting police and 1 of 9 months for threats to murder. A significantly higher proportion of all psychopaths committed aggressive offences than controls ($\chi^2=4.85$. $P<.05$).

It seems probable that the aggressive psychopath is so crippled in all his social relations that he is only able to live by crime and his record therefore consists very largely of acquisitive offences. Among offenders in general, however, records consisting very largely of aggressive offences are not particularly rare. A record of this kind without acquisitive offences should suggest

that the man is at least in fairly steady employment or is an unusually successful chief, neither of which is compatible with aggressive psychopathy. In general the persistently aggressive offender is unlikely to be a psychopath; the aggressive psychopath is unlikely to have an aggressive record on paper. The fairly lengthy follow-up makes it possible to attempt an answer to two questions which were not considered before. First, is the criminal psychopath distinguished in any way by the rapidity with which he relapses?

TABLE VII

Percentage of Total Number Reconvicted at Various Periods after Release

		Months		Years						Total
		1	6	1	2	3	4	5	8	
Psychopaths	..	..	31	29	18	9	3	1	0	91
Controls	..	..	14	3	7	7	14	1	7	53

Table VII shows the percentage of the groups of psychopaths and controls which relapsed in each year of freedom following release from the original sentence being served in 1948. The psychopaths relapse very rapidly. The most interesting feature of the table, however, is that the controls behaved in an unexpected way. Most groups of offenders who have been studied, especially Borstal lads or young adults (Mannheim and Wilkins, 1956) who make up the bulk of offenders, show a speed of relapse which resembles that of the psychopaths, i.e. the majority of the relapses occur in 6 months or a year, and thereafter much more rarely. One would expect the same pattern in the controls. In older normal offenders, however, this pattern may not be so characteristic, and part of the explanation of this behaviour of the controls here may be that their ages advance throughout middle life.

The second question, suggested by the high proportion (25 per cent.) of psychopaths who did *not* commit more than one minor offence, is: Does the diagnosis of psychopathic personality contribute anything to the accuracy of prognosis? Is the number of previous convictions in the end the best available guide to future behaviour, regardless of the psychiatric diagnosis? In Table VIII is set out the subsequent convictions of all the psychopaths who had only between 1-4 previous convictions, compared with all the controls with 1-4 previous convictions. The result shows rather disconcertingly that there is little difference in the behaviour of the two groups: the difference in the proportion of psychopaths and controls who have one or no reconvictions is quite insignificant ($\chi^2 = \cdot 003$). Whatever the prognosis of the psychopath may be in terms of his mental state, his *criminal* prognosis appears to be very uncertain and not very different from that of any other man with the same number of previous convictions. This may serve to remind us that our knowledge about the relevance of mental abnormality to criminal behaviour is still rudimentary.

Little can be said of course about what happens to the psychopaths who do not commit further crimes. When there have been no subsequent convictions at all, we cannot avoid a lingering suspicion, in spite of the checks which have been made, that they are in some institution out of harm's way, perhaps physically ill. One case (removed from the series) with 14 convictions, mainly for fraud, died of gastric haemorrhage, after four years of freedom from crime soon after the end of the first follow-up. The most convincing cases are those with a history of occasional fines for petty offences, which proves that they are

TABLE VIII
Twenty-four Psychopaths with 1-4 Previous Convictions

Convictions	Nil	1	2	3	3+
Less than 1 year	6	3	1	1	2
1-2 years	—	1	1	1	—
2-3 years	—	—	1	2	2
4-10 years	—	1	2	—	—

Twenty-six Controls with 1-4 Previous Convictions

Convictions	Nil	1	2	3	3+
Less than 1 year	5	6	3	—	1
1-2 years	—	—	3	—	1
2-3 years	—	1	—	4	—
4-10 years	—	—	1	—	1

in the community. Some of these records suggest that they have found some adjustment in the no man's land of betting, gambling and living on immoral earnings and some seem to become alcoholic tramps. The following case raises other possibilities:

Age 36. Constitutional psychopath, predominantly aggressive, with head injury: 16 previous convictions for larceny, receiving and some violence. During the follow-up period he received two sentences of 4 years in rapid succession for larceny and receiving. He was released in 1955. After that he was only fined for speeding in December, 1955, and fined £1 for not having a licence in February, 1957. In November, 1957 he was summoned for not paying his National Insurance contributions. His defence was an unusual one, that he was a thief—"I have not worked for 14 months; I live by thieving . . . I have a long and distinguished career of crime."

Lest we should underestimate the mental health problems presented by psychopaths, the following case may be quoted:

Aged 36. Aggressive psychopath, extremely demoralized by repeated punishment: 17 previous convictions for larceny and shopbreaking; often with violence. During the follow-up period he was sentenced to one month for assault on the police, six months for larceny and finally 8 years preventive detention for factory-breaking. In 1957 the prison records stated that he had "directed all his energies to the task of being as unco-operative as possible. He has twice returned to the first stage for persistent misconduct. He has been insolent, destructive, abusive and there has been a show of personal violence." He refused to be interviewed: the medical officer reported that he was very negativistic and had prolonged periods when his behaviour showed evidence of gross regression. On these occasions he was wont to urinate on the floor of his cell, smear himself with his faeces and curl up in a corner in a withdrawn way. However these periods would cease quite abruptly and he would become co-operative, smiling and apparently normal once again. The onset of these phases did not appear related to anything specific.

SUMMARY AND DISCUSSION

The criminal records of 72 severely psychopathic criminals and 59 control prisoners have been followed up for 8 years. As expected, the psychopaths had a significantly higher rate of reconviction than controls, but the surprising feature was that no less than 24 per cent. of the psychopaths had one or no reconvictions. A search has established that they are alive and not in mental hospitals. Those who were not reconvicted were mainly inadequate psychopaths; aggressive psychopaths almost always offend again.

The EEG was abnormal much more often in the psychopaths than the controls, but an abnormal EEG does not in general give any indication of the prognosis. In inadequate psychopaths, and those over 25 years old, however, there was some evidence that an abnormal EEG is a favourable sign, perhaps because it indicates the hope of a change by maturation.

Study of the crimes committed shows that although psychopaths committed significantly more aggressive crimes than the controls, aggressive psychopaths are much more often convicted of acquisitive than aggressive crimes: only 18 out of 105 subsequent convictions were for aggressive crimes. The offender with a record of exclusively aggressive offences is probably not a psychopath, and an aggressive psychopath usually has a record mainly of acquisitive offences, probably because his social relations are so crippled that he can only live by crime.

When speed of relapse was studied, the psychopaths were found to behave like most other groups of offenders who have been studied; the majority of the relapses occurring in

or 12 months and thereafter with rapidly decreasing frequency. The controls, however, behaved in a most unusual way in being reconvicted at a more even rate throughout the years of the follow-up.

Lastly, a comparison of the reconvictions of those psychopaths who had only 1-4 previous convictions, with those controls who had similar previous convictions, showed that there was no significant difference in their criminal behaviour. The diagnosis of psychopathic personality did not appear to contribute anything to the criminal prognosis; previous convictions seem to be a surer guide to prognosis than psychiatric diagnosis. This disconcerting finding raises a number of issues, and certainly shows how much progress still has to be made in understanding the relation between mental abnormality and criminal behaviour. It might be supposed that the diagnosis of psychopaths depended more heavily upon a history of previous convictions than was intended; but that is certainly not the case here, for Table VIII shows that a third of the cases were so diagnosed on general psychiatric grounds, although they only had 1-4 previous convictions. It is possible that the pattern of behaviour which leads to convictions and imprisonment is one that is rather specific, and may be learnt and unlearnt in ways which are relatively independent of all other aspects of the case, as criminologists have often maintained. The moral is that although a psychopath may continue to be a psychopath, whether he expresses this abnormality in the *criminal* field is another question.

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A NOTE ON THE CONCEPT OF DEMENTIA PARALYTICA AT THE TIME OF ROBERT SCHUMANN'S DEATH IN 1856

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IN a recent paper Slater and Meyer (in press) have pointed out that the nature of Robert Schumann's final psychiatric illness has never been satisfactorily explained. Moebius (1906) diagnosed it as dementia praecox, while Gruhle (1906) favoured an organic disease of the brain, probably paretic dementia. Subsequent opinion has been evenly divided between these two diagnoses but recent writers have inclined more to a diagnosis of schizophrenia. Slater and Meyer, however, on critically re-examining the available evidence, have definitely come down on the side of Gruhle.

Among the reasons given by Moebius against dementia paralytica was his contention that at the time of Schumann's death in 1856 knowledge of this disease in Germany was scanty. He based his opinion on a letter written to him by Dr. Pelmann, which indicated that Dr. Richarz, Superintendent of the Mental Hospital at Endenich in which Schumann spent the last two years of his life, a pupil of the psychiatrist Jakobi, could hardly have studied this disease in any detail, for only through the work of F. Hoffmann in 1861-1863 did knowledge of the disease spread in Germany.

It was, therefore, thought of interest to investigate the state of knowledge of dementia paralytica in Germany which was available to Richarz in 1857 when he contributed details of Schumann's illness to the first edition of von Wasielewski's biography and when later he published an article on the same subject in the *Kölnische Zeitung* (Richarz 1873). It is not difficult to obtain an accurate picture of the developing situation, as the history of dementia paralytica has been given in detail in reviews which include those of Salomon (1863), Mickle (1880), von Krafft-Ebing (1894) Mönkemöller (1911), Kraepelin (1913) and Zilboorg and Henry (1941). Original papers have been consulted as far as available and necessary.

REVIEW OF RELEVANT LITERATURE

Although according to Zilboorg and Henry cases of paretic dementia had definitely been observed by Haslam (1798), Cox (1804) and Esquirol (1805), and possibly even by Thomas Willis as early as 1672, the merit of having introduced the concept of the new disease based on clinico-pathological investigation of a large number of patients belongs by general consent to Bayle (1822; 1826) and Calmeil (1826). By 1826 Bayle had already investigated nearly 100 cases both clinically and pathologically. Both these writers speak of "*la paralysie générale incomplète*", to distinguish the widespread bilateral but at first only slight disturbances of motor function, in particular of speech and gait, from the unilateral complete paralysis of the contralateral side

observed in compression of the brain by softening, tumour, abscess, tubercle and haemorrhage. This description was accepted by many French authors, including Delaye (1824) and Parchappe (1836). The unitary concept of the condition held by most of these authors was temporarily abandoned by Maillarger (1847) and others, who distinguished separate forms of general paralysis with and without insanity. However, the entity was restored by Delasiauve (1851) and Falret (1853).

Most French authors considered the pathological lesions to be inflammatory in nature. Salomon, a Swede whose translated paper was published in the *Journal of Mental Science* in 1863, contrasted this French emphasis on inflammation with the somewhat later German interpretation of the pathology as primary degeneration. It should be borne in mind, however, that the macroscopic and histological concept of inflammation at this time was still ill-defined and naturally far removed from modern aetiological connotations. Thus terms such as chronic arachnitis (Bayle), diffuse meningo-encephalitis (Calmeil), chronic encephalitis (Georget 1823), chronic cortical cerebritis (Parchappe) and inflammatory induration of cortex and white matter (Foville 1829) did not have their present specific meaning. For example Bayle (1826) believed that inflammation of the brain could be produced by "moral" as well as physical causes. He explained the predominance of males afflicted with *la paralysie générale incomplète* by their greater exposure to mental shocks, head injury, alcoholic and venereal excesses, violent passions, etc., a theme which long dominated discussion of the disease both in France and Germany. Moreover, sponsors of the inflammatory hypothesis were not entirely lacking in earlier German publications on this subject.

The situation in Germany and German-speaking countries is described in the reviews already mentioned and in a paper by C. Westphal translated into English and published in the *Journal of Mental Science* in 1868. However, the principal witness, especially for the earlier period up to 1850, is Wilhelm Griesinger, one of the great names in psychiatry of that time in Europe, who is known to have been in personal contact with French psychiatrists. Brief but definite references to the disease and to the relevant French literature are found in two of Griesinger's articles dated 1844 (vide Griesinger 1872). The second article is of particular interest here since it was a critical review of a book by Jakobi (Dr. Richarz's teacher). Griesinger wrote: "As regards prognosis the author [Jakobi] agrees with Calmeil, Bayle, Esquirol, and others that the mental patients who suffer from paralysis usually die in the course of the same year in which these appearances are first observed. Others, including ourselves, have seen them live for several years in good care" (p. 102; translation).

The first edition of Griesinger's famous book *Die Pathologie und Therapie der psychischen Krankheiten* appeared in 1845 and must have been widely read. In this there is a full account of the "*sogenannte allgemeine (unvollständige) Paralyse*" (pp. 261-286) which according to the author deserves the closest attention because of its frequency, its peculiar course and its most unfavourable prognosis. Previously the condition had been studied exclusively by French physicians (Esquirol, Calmeil, Bayle, Delaye, Foville, etc.) with whose descriptions his (Griesinger's) experiences agree. He proceeded to recount with surprising detail and lucidity the clinical symptomatology: the characteristic disturbance of speech, gait and "precision movements" such as writing, sewing, etc.; the epileptiform seizures during periods of congestion of the head; the exaltation with "*monomanie des grandeurs*" and—more rarely—

melancholic states; the progressive deterioration of memory, social habits and "mental combination", until finally complete dementia is reached after a course of six months to three years, a longer duration being exceptional. Short remissions are frequent. Men are more frequently affected than women, possibly because they are more inclined to alcoholic and sexual excesses (spermatorrhoea).

The pathology is treated mainly on pages 318-319. Quoting Calmeil, Bayle, Parchappe, etc, Griesinger states that the great majority of cases show softening and decomposition of cerebral cortex with firm adherence to the pia mater and often induration of the white matter. The brain is atrophic with excess of fluid in the meninges and in the enlarged ventricles, which contain granulations in their hardened walls. He still reserved judgment as to the specificity of the pathological lesions.*

In 1861 Griesinger gave an even fuller and in certain respects more definite account, and was able to refer to a number of German papers which in the intervening decade had amplified the original French concept of "*la paralysie générale incomplète*" or, as it came to be known, "*paralytischer Blödsinn*", "*allgemeine (progressive) Paralyse*" or simply "*Paralyse*".

The first major contribution in German seems to have been made by Duchek (1851) who regarded the condition as one of chronic inflammation of the meninges resulting in atrophy of the brain. Other publications, not all available to the present writer, included those of L. Hoffmann (1850), Joffe (1857), Erlenmeyer (1857) and L. Meyer (1858); the latter described capillary changes in meninges and cortex associated with inflammatory perivascular cellular proliferation. Considerable attention was paid to histological investigations by Rokitansky (1857) and Wedl (1859). Rokitansky described connective tissue fibrosis of the cerebral cortex, the ganglion cells being dissolved and changed into "colloidal bodies". Likewise Wedl saw in all cases of paretic dementia hypertrophy of connective tissue in small arteries and veins in pia mater and cortex leading to obstruction of the lumen and occasional calcification with consequent disturbance of the circulation, ischaemia, stasis, pressure on the parenchyma, irritation and inflammation.

Summarizing the anatomo-pathological findings, Griesinger (1861) stated that the majority of these cases showed as the most frequent change oedema of the meninges, adherence of pia mater to the surface of the cortex and softening or induration of cerebral cortex with loss of the nervous elements, extending down into the spinal cord. Although inflammation could not be entirely excluded, cerebral irritation, congestion and defective nutrition of the brain substance were more frequent as primary lesions. He dismissed syphilis as a cause of the disease, and like Bayle explained the predominant occurrence of paralysis in men by their alcoholic and venereal excesses and their addiction to "strong cigars and strong coffee". Salomon even more explicitly stated as the prevailing German view that "the primary cause, therefore, is degeneration of the vascular walls. Hence proceeds derangement of the circulation with its consequent disturbed nutrition". This interpretation was maintained by Mickle who thought that over-activity and strain induced hyperaemia resulting in embarrassment of the circulation in the brain. Exhaustion from over-excitation of the cortical nerve cells might also induce paralysis of the vasoconstrictors followed by overgrowth of the vessel wall cells

* The second (1851) edition of Romberg's textbook also contains a review of French publications on General Paralysis, quite separate from the author's famous description of *tabes dorsalis*.

and of the neuroglia. The nerve cells suffer from lack of nutrition and degenerate, and finally mild inflammation may supervene.

The history of the aetiological role of syphilis in dementia paralytica makes interesting reading; a particularly full and accurate account has been given by Hougberg (1894). Esmarch and Jessen (1857) probably made the first definite claim for syphilis in parietic dementia and they were supported by Steenberg (1860), Kjelberg (1863, *vide* Kjelberg, 1868) and later by Jespersen (1874), all Scandinavians. During the next twenty years, however, this aetiological concept was not favourably received: most German psychiatrists, among them leading figures such as Griesinger (1861), L. Meyer (1861), C. Westphal (1863), Schüle (1886), more or less rejected it. In France, the official attitude was even more negative, with the sole exception of Fournier, who between 1875 and 1878 suggested a syphilitic origin for tabes and pseudo-paralytic dementia, but explicitly excluded true general paralysis (Fournier 1894). Convincing statistics in greater numbers did not appear until after 1880; Hougberg mentioned as the most important those by Mendel (1880), Rieger (1886), Rohmell (1884) and Binswanger (1891).

Nevertheless, the problem remained hotly disputed in discussions at the International Congress in Copenhagen in 1884 and at similar meetings in other countries (*vide* Hougberg). In his *Compendium of Psychiatry*, Kraepelin (1883) mentioned syphilis as a particularly important cause among others, such as trauma, insolation, chronic alcoholism and psychological stress. Syphilis, he argued, may produce a chronic interstitial encephalitis or may merely reduce the resistance of the brain. Krafft-Ebing, while adducing new immunological and statistical evidence in favour of syphilis, still relegated the latter into the category of "accessory causes" together with trauma (*italicized!*) and insolation. For him parietic dementia was a disease occurring at the peak of life precipitated by excessive mental responsibilities and sexual and alcoholic extravagances. He conceived parietic dementia to be the disease of the nineteenth century, with its rapid growth of civilization; psychoses which in earlier times remained "functional" now—under the influence of the damaging by-products of civilization—developed into organic brain diseases, while from his own experience in Vienna psycho-neuroses had become progressively rarer. It is a pathetic reminder of the limitations of human judgment that this was written by a psychiatrist of distinction only one year before Breuer and Freud (1895) published in the same city their studies on hysteria.

While Nonne (1909) still believed that syphilis was not a *sine qua non* of paralytic dementia, Kraepelin accepted it as an essential cause in 1904, the year in which Nissl and Alzheimer also produced what—apart from the later demonstration of spirochaetes in the brain—has become the definitive description of the histopathology of the disease. But even Kraepelin postulated noxious factors of civilization as at least a contributory cause and also adhered to the concept of the disease as para- or meta-syphilitic, concepts which continued to haunt German psychiatry for a considerable time.

THE REPORTS OF DR. RICHARZ

It is in the light of this background that Richarz's two reports on Schumann's illness must be studied. The relevant passages in his contribution to Wasielewski's biography in 1857 run as follows: "I comply willingly with your wish to obtain from me some information on the nature of Robert Schumann's disease and cause of death. The best procedure is to begin with

the autopsy findings as a certain and objective basis. . . The main results of the postmortem examination were, as was to be expected, derived from the brain. . . Its most important abnormalities were, in the order of increasing importance, the following:

- (1) Congestion of all blood vessels, particularly at the base of the brain.
- (2) Proliferation of bone . . . with new formations of bone which partly penetrate the dura mater with their pointed ends.
- (3) Thickening and degeneration of both inner (lepto-) meninges of the brain and adherence of pia mater to the substance of the cerebral cortex in several places.
- (4) A not inconsiderable atrophy of the brain as a whole, the weight of which was almost 7 ounces . . . less than appropriate to Schumann's age.

"These four points are in intimate connection with the psychic states which had persisted . . . for many years; they indicate jointly a very severe disease of the whole personality which has its intangible origin, as a rule, at an early period of life . . . and after long preparation culminates in manifest insanity. . . The clumsiness of speech which had been observed for a long time is, as a rule, the first of the paralyses characteristic of this disease. One of the most important external causes of the disease is mental fatigue and excessive psychical activity or extravagance . . . a danger to which artistic and particularly musical activity is easily exposed. There is no doubt that such excesses have occurred in Schumann's life. . . For a certain time the brain, as every other excessively active organ, is supplied with a correspondingly increased volume of blood. The next consequence, however, is dilatation of blood vessels, thickening and degeneration of the meninges, adherence of meninges (particularly pia mater) with the brain substance . . . and interference with blood supply and reduced nutrition of the brain resulting in atrophy.

The mental disturbance caused by this organic brain disease is always in the nature of dementia, i.e., progressive loss of intelligence which in Schumann reached a higher degree only late. . . The emotional state is usually that of exaltation . . ." (translation).

In 1873 in order to contradict rumours of hereditary insanity in Schumann's family Richarz again wrote as follows: "Schumann's last and fatal illness was not, as might be inferred from certain symptoms, an inherited specific dementia; rather it was characterized by slow organic but persistent decline of the organization and vitality of the whole nervous system, in the form of *incomplete paralysis* ("*unvollständige Paralyse*", the present writer's italics), of which the mental alienation was merely a partial manifestation. Apart from some inherited germ such as may be present in all human beings, the disease is always the result of wear and tear caused by excessive mental, and particularly artistic, activity . . . the expenditure of the substance being quicker than its replenishment. . . Intense cerebral effort must be regarded as the most certain cause of this terrible affliction which is inaccessible to treatment . . ." (translation).

CONCLUSIONS

The following conclusions can be drawn from this brief and incomplete historical review of the development of the concept of parietic dementia and its comparison with Dr. Richarz's medical reports.

(1) Contrary to Pelmann and Moebius there is clear indication that as early as 1844 Griesinger and Jakobi knew Bayle's, Calmeil's, Esquirol's, 'Archappe's and other French authors' writings on *la paralysie générale*, although at that time they had apparently not yet fully accepted it as a clinico-pathological entity. They had certainly observed and diagnosed patients suffering from this disease. In 1845 Griesinger gave a surprisingly full and vivid account of "*die allgemeine (unvollständige) Paralyse*" based upon detailed knowledge of the French publications and on personal experience. Important original contributions on the subject in German began to appear from 1851. By 1873, the date of his second report, Richarz could not but have taken cognizance of a considerable number of German publications which included Griesinger's widely read book in its two editions and Westphal's publication. Richarz's interpretation of Schumann's disease in 1873 was in all essentials identical with that of 1857. By no means can the later report be taken to be a correction of his 1857 diagnosis.

(2) The wording in the description of clinical as well as postmortem findings by Richarz follows so intimately that used by the earlier French and contemporary German writers that it is hard to believe that it was carried out without knowledge of these publications. The clinical combination of speech disturbance and elation with progressive dementia, the postmortem findings of congestion, thickening and degeneration of the meninges and their adherence to the atrophic brain, and the pathogenic sequence of mental overstrain and other excesses, repeated congestion, interference with blood supply and lack of nutrition of the brain substance, etc., are so strikingly reminiscent of the descriptions and hypotheses of Bayle, Calmeil, Griesinger and others that Richarz must have been familiar with these and not only, as Moebius suggests, Esquirol's saying "*l'embarras de la parole est un signe mortel*".

It is interesting that the word "inflammation" is not used in either report. If one follows Salomon's distinction between the French emphasis on inflammation and the German on degeneration, one might conclude that Richarz had adopted the German view. As has been pointed out, however, this contrast is not nearly as distinct as it appeared to Salomon.

Syphilis as a possible cause is not mentioned in either report of Richarz, which is not surprising as between 1857 and 1873 this suggestion had not been accepted by German psychiatrists. It is, therefore, not likely that Richarz hinted at this aetiology when he spoke of the "excesses" to which artistic activity is exposed.

(3) If doubts should still linger as to Richarz's identification of Schumann's fatal illness with the disease described by Bayle, they appear to be completely removed by the one addition in his 1873 report: "in the form of incomplete paralysis". At first it was assumed by the present writer that this was intended by Richarz to draw attention to a lesser degree of dementia than is usual in this condition. It is far more convincing to assume that by adding these few words Richarz directly referred to Bayle's and Calmeil's concept of *la paralysie générale incomplète*.

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MENTAL HOSPITAL ADMISSIONS IN MYSORE STATE, INDIA

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THE older psychiatric literature has here and there valuable reports about mental illnesses amongst African, Asian and other societies which are very different from Europe. These authors normally express opinions or speculations based on chance experiences rather than systematic observations. It is only in recent years that more extensive mental health work has been done in these countries, which also has enabled many workers to collect a great deal of information. This is important not only for the countries concerned, so as to help them to plan an appropriate health service, but also for psychiatry as a whole. It gives an impetus to comparative psychiatry, one of the most important basic sciences to clinical psychiatry.

We are not aware of any systematic psychiatric investigation having been carried out in India. There are a large number of opinions expressed on mental illness in that country, mostly based on studies of a few individuals, sometimes only on preconceived notions of the author. These studies are interesting. They have often been compared by critics to the best in English fiction.

The present report is an analysis of certain factors which it has been possible to extract from the case reports of all admissions to the Mental Hospital in Bangalore, Mysore State, in 1953. In respect of some of these factors the patients could be compared with the general population of Mysore State. A few of the findings allow, perhaps, certain limited, tentative conclusions.

DESCRIPTION OF THE HOSPITAL

The hospital, which has officially 400 beds but contains up to 500 patients, is housed in a relatively new building, situated on the outskirts of Bangalore in a beautiful park. It has two wings, one for male and one for female patients. Indian custom demands that these should be kept entirely separate. The administrative part of the hospital and the two wings embrace an attractive garden which is enclosed on the fourth side by a row of bungalows for private patients.

The hospital is the only in-patient unit for psychiatric patients for the whole state of Mysore. Mysore has 9,074,972 inhabitants and an area of 29,489

square miles. Distance plays an enormous part in limiting the services the hospital can render to the population if it is borne in mind that the transport system in the state, although better than in other Indian states, is not very elaborate. Patients often have to travel by jetka* or on foot for many miles. As many patients who come to the hospital for admission are acutely disturbed they are brought in chained by their relatives, having marched at the end of a chain or rope for 30 or 40 miles. There is an ambulance service, which, however, is quite inadequate to deal with the enormous problem.

METHODS

The information about the patients was obtained from the official hospital records. These are filled in by the medical officers.

The information about the State of Mysore was taken from the official Census Report of 1951. There is no reason to believe that the figures for 1953 would be materially different. As the Indian Government has not continued the tradition of previous censuses to list caste and break figures down according to caste, this information was obtained from the previous Census Report of 1941. Again there is no apparent reason to assume that these figures have materially altered.

RESULTS AND DISCUSSION

TURNOVER

The hospital is a very active one, as can be seen from Table I. The table

TABLE I

Catchment Area	Sex	Population	First Admissions	First Admissions per 100,000 of Population
Mysore State First Admissions	M	4,657,409 (51·3%)	379 (61·5%)	8·11
	F	4,417,563 (48·7%)	253 (38·5%)	5·73
	Total ..	9,074,972	632	6·96
Mysore State , Re-admissions ..	M		112 (62·6%)	
	F		67 (37·4%)	
	Total ..		179	
Mysore State Total	M		491 (60·6%)	10·5
	F		320 (39·4%)	7·3
	Total ..		811	8·95

The table shows First and Re-admissions from Mysore State alone, giving the figures for 100,000 of the population.

only includes the patients who come from Mysore State, the proper catchment area of the hospital. In actual fact the hospital admits a large number of patients who come from outside the borders, often from very far afield. The hospital

* jetka—a small horse-drawn two-wheeler.

as a teaching institution enjoys a great reputation and attracts many who are in need of help. For the purpose of this report we have only examined the numbers pertaining to residents of Mysore State.

There were a total of 811 admissions in 1953 of which 491, or 60·6 per cent., were men and 320, or 39·4 per cent., were women. This amounts to an admission rate of 8·95 per 100,000 of the population. Of these admissions 632 were first admissions. This amounts to 6·96 per 100,000 of the population. One hundred and seventy-nine re-admissions, or 22·5 per cent. of all admissions, is a figure only moderately different from that for England and Wales where it was 39·5 per cent. in 1952-53. The first admission rate in relation to the population is however quite different from figures in England and Wales or in any European country or the United States. For England and Wales the figure is 97 first admissions for 100,000 population; in the United States it is even higher. This is not surprising, as admission rates to hospital depend on a great many factors which differ not only from country to country but within countries from hospital to hospital. As an example we need only refer to the report on Graylingwell Hospital (Carse *et al.*, 1958). These authors report a fall in admission rates to hospital within two years from 548 to 229, i.e. by 59 per cent. They attribute this change to certain alterations in the mental health services in the area. Another example is the report by Shepherd (1958) which shows that the relation between admission rates in a particular hospital have changed in the course of 10 years. He too sees the main reasons for the change in alterations within the mental health service in that area. The examples could be multiplied.

Indeed the Mental Hospital, Bangalore itself provides an illustration of much change having occurred in its own history. Table II shows the enormous

TABLE II
Mental Hospital, Bangalore

Year				Admissions	Discharges and Deaths	Admissions to General Hospitals in Mysore State
1923	..	..	..	103	127	
1930	..	..	..	164	179	
1940	..	..	..	495	486	70,955
1945	..	..	..	535	531	97,040
1953	..	..	..	948		

This table shows a rapid increase in admissions to all hospitals, but a much sharper rise in admissions to the Mental Hospital.

increase in numbers over a relatively brief period. Whereas 1923 showed only 103 admissions this figure has increased five times only 22 years later to 535, and has multiplied nearly 9 times in 1953. This rapid increase is still taking place and the figures for 1956, which are not included in this paper, are 15 times those of 1923.

SEX

Of first admissions 379, or 61·5 per cent., are men and 253, or 38·5 per cent., are women (Table I). This ratio is the same as regards re-admissions where the respective figures are 112, or 62·6 per cent., men and 67, or 37·4 per cent., women. This proportion is not the same as in England and Wales. Here the ratio is almost reversed, being, for first admissions, 42·6 per cent. men and

4 per cent. women. But although these figures are different from the figures in the U.K. they are very similar to those found in the U.S.A.

The admission figures in relation to the population are very much lower than those in European countries. The population has more males than females, the figures being 51·3 per cent. males and 48·7 per cent. females, i.e. 949 males per 1,000 females. The admission figures indicate that there is a higher admission rate amongst males with 8·11 per 100,000 of the male population compared with 5·73 per 100,000 females. The corresponding figures for England and Wales are 86 male and 107 females per 100,000 of each sex. It is of course difficult to be certain about the causes for these differences, but certain possible explanations spring to mind.

Most of the patients admitted are in a state of acute disturbance. We believe that owing to the attitudes towards mental patients amongst the population, one of the main indications for admission to hospital is violent or hysterical behaviour. If that is so it is obvious that female patients can be more easily controlled at home. On the other hand, it is also possible that the family goes to greater lengths to restore the health of the breadwinner rather than the health of a female member of the family. This will be discussed in more detail later.

BE

The age distribution of the patients is perhaps one of the most important items of information for the psychiatrist, but it is also the most difficult to come by in India. Most patients do not know their age and make a guess, or the doctor makes his own guess. An analysis of the ages entered in the records shows a suspicious clustering of the last digit at 0 or 5. It would appear that there are always a much larger number of persons at these round ages than at any others. There is no way of checking on the exact age of any patient even if the doctors were prepared, or had the time, to do so, as the age is just not known. Registration at birth is not compulsory and many births are never notified. Very few possess birth certificates. Certain persons—a minority who can afford it, notably Brahmins or other high caste persons—have a horoscope which states the time and date of birth. But the date usually refers to one of the 10 or more different calendars in use in India and, even if committed to memory, would require a good deal of trouble were it to be translated into the Gregorian calendar.

Various attempts have been made by clinicians to get accurate information about the age of their patients. One can ask an older relative—who usually will not know the age any more than the patient—to relate the birth to a well-known event, e.g. one would ask was he or she born before or after the shelling of Madras by Japanese ships, and how many years before or after. But the limitations of such a method are obvious. Various workers have tried to assess age by physiological methods such as X-rays of the bones. But it was shown by Modi (1952) that the average rates of ossifications vary greatly from district to district of the sub-continent. This means that the method totally lacks standardization and has therefore no validity at present.

The ages, as shown in these tables, must therefore be regarded sceptically, and are only presented for lack of anything more accurate.

Table III shows that the majority of patients in both sexes come from the age groups between 15 and 34, to a lesser but still high proportion from the age groups between 35 and 44. The figures are 412 (65 per cent.) admissions

TABLE III

Age Group	Population	Male		Population	Female		Population	Total	
		First Admissions	Per 100,000		First Admissions	Per 100,000		First Admissions	Per 100,000
5-14	118,855	8	0.672	120,402	10	0.834	239,257	18	0.752
15-24	84,987	140	16.45	79,116	74	9.35	164,103	214	13.1
25-34	71,479	107	15.00	69,198	91	13.2	140,677	198	14.2
35-44	57,261	67	11.67	46,914	44	9.40	108,675	111	10.2
45-54	49,753	33	6.62	33,708	23	6.82	83,461	56	6.71
55-64	22,653	21	9.52	19,569	11	5.62	42,222	32	7.59
65-74	8,876	3	3.39	8,058	—	—	16,934	3	1.77
All ages from 5-74	413,864	379	9.17	376,965	253	6.70	795,329	632	7.94

The Age Groups in this table are derived from a 10 per cent. sample of the population. The number included in the sample are 905,857. The Admissions are from Mysore State only.

between the ages of 15-34, a further 111 (17.6 per cent.) between 35 and 44, and only 91 (14.4 per cent.) for all ages above 45. The proportions remain approximately the same for the sexes separately i.e. in the 15-34 group there were 247 (65.2 per cent.) male and 165 (64.6 per cent.) female first admissions; in the 35-44 group there were 67 (17.7 per cent.) male and 44 (17.2 per cent.) female first admissions, and in all age groups above 45 there were 57 (15.1 per cent.) male and 34 (13.3 per cent.) female admissions. These figures are certainly in striking contrast to the findings in other countries, e.g. the U.K. (the Registrar-General's Report 1952-53, p. 7). In England and Wales the percentage for the 15-34 age group is 25 per cent. (male 24.8 per cent., female 24.9 per cent.), and for the age group 35-44 it is 16.9 per cent. (male 16.8 per cent., female 16.9 per cent.) and for all ages above 45 years it is 55 per cent. (male 51.8 per cent., female 57.2 per cent.). It can be seen that more than half of all first admissions come from the older age groups. This is the reverse of the situation in Mysore State. The figures are however not strictly comparable. The graphs in Figure 1a, 1b which were drawn from figures given in the Mysore Census Report (1951, p. 100) show certain differences although the differences are less than might be expected.

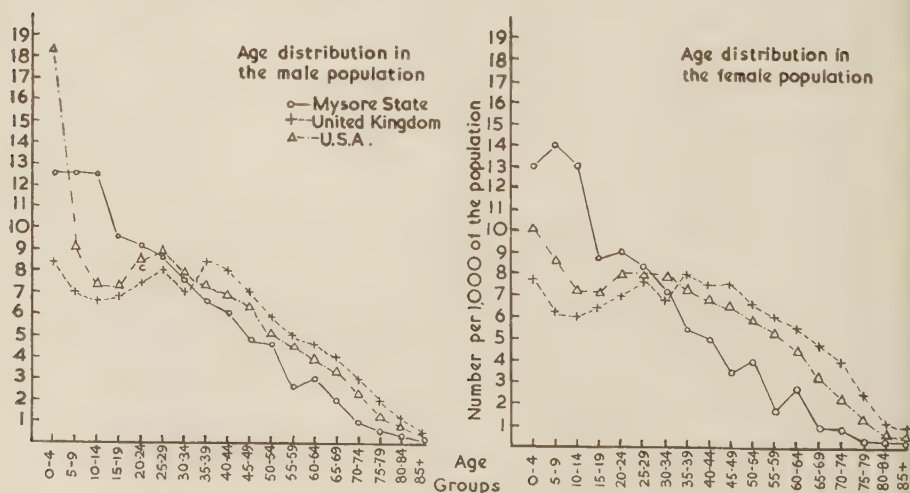


FIG. 1 a, b.

REMARKS CONCERNING THE CENSUS REPORT

It will already be clear from what was said above on obtaining information on age in patients, that Census findings will hardly be more accurate. The

census officials are dealing with large numbers and cannot spend nearly as much time with each person as can the doctor. The reasons for these difficulties in obtaining information on age are manifold. In addition to the fact that most persons do not know their age, many people who do know dislike disclosing it. We are not referring here only to this reluctance so prevalent amongst the ladies—inside and outside India—and certain age groups in men; the *Upanishads*, one of the ancient Hindu tracts, contains an injunction against the disclosure of a person's age and will influence the religious. The purpose of this injunction is not clear. It may be connected with the belief prevalent among the orthodox that telling one's correct age is equivalent to boasting and reduces the span of life. It is also believed that a man by giving his true age places thereby an opportunity into the hands of his enemy to unleash against him the forces of black magic. Superstition against certain numbers such as 13 is probably also a source for misstatements. Nor do the dangers which may arise from disclosing one's true age always lie in the realms of mysticism or magic. The Mysore Government initiated a probe into the dates of birth of their officers which brought to light a large number of understatements of age, made with the main intention of postponing the date of retirement. Similarly, there have been cases where persons tried to get over the age restrictions for recruitment by giving a false age. The Mysore Census Report (1951, p. 98) sums up the situation, "It would be clear from the above remarks that our age-returns are far from being trustworthy." It ends however on an optimistic note, "Yet the observations of tendency and the facts of probability make it possible to draw a greater value from the returns than might be expected."

FIRST ADMISSION RATES BY AGE GROUPS PER 100,000 OF THE POPULATION

Table III shows the figures extracted from the Census Report of 1951. As the numbers are not expected to have changed greatly between 1951 and 1953 they are used here to calculate admission rates in relation to population. These figures are derived from a 10 per cent. sample of the population. The tendencies observed above still hold when the admission numbers are calculated in proportion to the corresponding age group in the population. The actual figures are 13·4 new admissions per 100,000 of the population in the 15–34 age groups, 13·2 new admissions per 100,000 of the 35–44 age groups, and only 6·3 for all ages above 45. Again the proportions are the same for the sexes. It is interesting that the highest proportion of female admissions occur in the 25–34 age bracket, namely 13·2 per 100,000, as opposed to only 9·35 in the 15 to 24 age group. It must be remembered that the vast majority of marriages are consummated shortly after the menarche which occurs at about the same age as in the U.K. and that a large number of children are born to mothers in the 15–24 age group. It appears from these figures that early childbearing does not seem particularly to increase the risk of admission to a mental hospital. As far as this is an indication for the incidence of mental illness, very early childbearing would not appear to increase the mental morbidity risk in the mothers. This has also been observed in the U.K.—in this case unmarried mothers—(Anderson *et al.*, 1957, Fairfield 1940). Again the figures show a striking difference to those applying to England and Wales (Registrar-General's Report, 1952–53, p. 7). Here the first admission rate per 100,000 of the home population is 536 in the 16–34 age groups, 227 in the 35–40 age groups, and 1,350 in all ages over 45. The female rates in the over 20 age groups are consistently higher than the male ones. They are 375 per 100,000 in the 16–34 group, 129 in the 35–44 group, and 715

TABLE IV

District	Population	Per cent. of Total Population of Mysore State	First Admissions	Per cent. of the Total Number of First Admissions	First Admissions per 100,000 of the Population
Bangalore ..	1,348,084	14.8	60	14.6	4.75
Bangalore Corp. ..	778,977	8.6	176	40.4	22.6
Mysore ..	1,040,448	11.4	19	4.6	1.93
Mysore City ..	244,323	2.7	30	6.5	11.5
Kolar ..	970,791	10.7	19	4.2	1.86
Kolar City ..	159,084	1.8	8	1.8	5.04
Tumkur ..	1,151,362	12.7	24	5.5	2.09
Mandya ..	717,545	7.9	44	10.2	6.12
Chitaldrug ..	868,370	9.6	12	2.76	1.49
Hassan ..	715,135	7.9	14	3.23	1.96
Chickmagalur ..	417,538	4.6	10	2.3	2.15
Shimoga ..	663,315	7.3	17	3.91	2.56
Mysore State ..	9,074,972	100.0	433	100.00	4.68

This table only includes Regular and Voluntary Patients. Observation cases are not shown.

in the above 45 age group. The difference between male and female admission rates remains almost the same for all age groups over 20.

DISTRICTS

Table IV shows the first admissions to hospital according to the district where the patient is domiciled. The admission figures could only include



FIG. 2—Map of Mysore State, 1951

untary and regular patients; the observation cases had to be omitted because address stated on their case records is often the location of the referring agency, usually a law court, rather than the domicile of the patient.

The table shows that the largest number of patients, namely 40·4 per cent., come from Bangalore Corporation. The next largest percentage comes from Bangalore district (14·6 per cent.) and from Mandya (10·2 per cent.). The smallest percentage comes from Kolar City (1·3 per cent.). But if the admissions are expressed in relation to the population figures it will be seen that the differences while still large are not nearly as wide as would appear. The highest proportion comes again from Bangalore Corporation with 22·6 per 100,000 of the population; but this figure is only 14 times greater than the smallest, 1·49, which applies to Chitaldrug. For absolute admission percentages the highest figure is over 22 times bigger than the smallest.

The largest proportion of admissions per 100,000 of the population is found in Bangalore Corporation with 22·6 per cent., next from Mysore City with 11·5, and next from Mandya with 6·12 followed closely by Kolar City with 5·04. Bangalore district comes next with 4·75. The other districts vary between 1·49 and 2·56, the figures being too small to allow too much meaning to be read into the differences. With the exception of Mandya, which is a very industrialized district, the highest proportion of mental hospital admissions is to be found in the large towns, the figures being proportionate to the size. This is not an unexpected finding, particularly if it is borne in mind that the type of patient referred to the hospital is the grossly disturbing rather than the disturbed patient. The size of towns seems even more important than the distance from hospital, as shown by the figures for Mysore City (11·5) which is very far away, compared with those of Bangalore District (4·75) which is closest to the hospital, next to the Corporation itself. The difference between Mysore City with 11·5 per 100,000, which puts it second on the list, and Mysore District with 1·93, placing it near the bottom of the list, is equally illuminating at this point.

Although distance from the hospital is not a factor of overriding importance, it does, nevertheless, influence the admission rates. This is not surprising considering not only how enormous these distances really are, but also the inadequate means of transport available. The actual figures are seen in Table V where the districts are grouped geographically according to distance from the hospital. The nearest group of districts shows 11·09 admissions per

TABLE V

Patients' Residence Districts are Grouped According to Distance from the Mental Hospital	Population	First	First
		Admissions (Voluntary and Regular Patients only)	Admissions per 100,000 of the Population
Nearest:			
Bangalore, Bangalore Corporation	2,127,061	236	11·9
Intermediate:			
Kolar, Kolar City, Tumkur,			
Mandya	2,998,782	95	3·17
Furthest:			
Mysore, Mysore City, Hassan,			
Chitaldrug, Chickmagalur, Shimoga	3,949,129	102	2·58
Mysore State	9,074,972	433	4·68

This table shows that distance from hospital influences the admission rate.

100,000 of the population, the next nearest group 3·17, and the most distant group 2·58.

SOCIAL AND ECONOMIC FACTORS

Table VI summarizes a number of social and economic factors, comparing them as they apply to the various districts. The factors are chosen because they might conceivably affect admission rates to the mental hospital.

TABLE VI

Districts	Per cent. of Population Living in Villages or Towns of:		Population per Hundred Living on Agriculture	Density		Persons per House	Average Annual Growth Rate Between 1941/51
	Less than 2,000	20,000 to 100,000		Crude	Cultivable Area		
Bangalore ..	76·7	3·28	74·2	441	969	6	25·6
Bangalore Corporation ..	—	Population over 100,000	15·7	—	—	9	Average for Cities 52·4
Mysore ..	71·5	—	84·5	294	515	5	13·5
Mysore City ..	—	Population over 100,000	7·9	—	—	7	Average for Cities 52·4
Kolar ..	73·8	4·76	82·1	307	783	5	14·6
Kolar City ..	—	Population over 100,000	12·3	—	—	6	Average for Cities 52·4
Tumkur ..	83·95	2·96	83·9	281	517	5	18·6
Mandya ..	79·92	2·96	85·0	374	595	5	12·1
Chitaldrug ..	72·90	9·35	76·0	207	342	6	17·8
Hassan ..	85·14	3·48	83·5	271	472	5	13
Chickmagalur ..	80·46	5·19	72·9	150	427	5	15·3
Shimoga ..	73·38	13·52	71·2	164	414	6	18·2

(a) *Size of Village or Town of Residence Prior to Admission*

This column shows the number of persons in the various districts who live in small villages. It will be seen that the majority of the population still lives in villages with populations of less than 2,000. It might be expected that the districts where the largest populations live in the larger towns would show a higher admission rate per population. This however does not appear to be so, except in the case of the three large cities, namely, Bangalore Corporation, Mysore City, and Kolar City. If this factor also applied to the other districts, Shimoga and Chitaldrug should have the highest admission rates, and Mandya and Mysore district the lowest. In actual fact Shimoga occupies third place and Chitaldrug the last, whereas Mandya shows first place and Mysore District seventh. The size of the town only appears to be a factor if the towns are *very* large as in the case of the three cities.

(b) *Agricultural versus Industrial Occupation*

Agricultural versus industrial occupation amongst the population might conceivably play a part, but this again is not confirmed by the figures. If industrialization was in fact increasing admission rates Shimoga and Chickmagalur should stand at the top of the list. In fact they come in third and fourth place; Mandya and Mysore district should be lowest but show up in first and seventh place respectively, that is not counting the three large cities.

(c) *Density of Population*

Mysore State has 308 persons per square mile. This compares with Europe as follows. Holland with 717 and Belgium with 715 are the most densely

populated countries, Sweden with 38 falls among the less densely populated countries, with U.K. at 550 in the middle. The All India density is less than in Mysore, namely 281. But among the Asian countries India ranks very high. China has a density of 123, Pakistan only 96.

As large parts of Mysore State are uncultivable land the crude density however is less likely to show a correlation with admission figures than the density in relation to the cultivatable area in each district. However, no correlation could be found with these figures either. According to density Bangalore district and Kolar district should show the highest admission rate, but they come in second and eighth place respectively. Shimoga, which should be near the bottom of the list, has in fact third place.

(d) *Number of Persons per House*

In other words, crowding could be expected to increase the admission rate. As the table shows, however, there is hardly any difference between the districts as far as that is concerned.

(e) *Average Annual Growth Rate between 1941 and 1951*

This might conceivably be expected to show a correlation with the admission rate, affecting it indirectly, but the table does not show any such correlation clearly except in the case of the three large cities. But Mandya, which has the lowest growth rate, shows a high admission rate. Nevertheless there is a better approximation between the admission rates and the growth rate than is shown in any of the other columns.

MARITAL STATE

The Tables VIIa and b show the relationship between admission rates to the mental hospital and marital status. The figures are further broken down according to age groups.

The attitude to marriage in India is very different in many ways from that in European countries. There is a difference in Mysore State between the various communities such as the Hindus, the Muslims or Christians, but in spite of the differences there is also a great deal in common.

In the Muslim community polygamy is tolerated. In the Hindu community the Government introduced a law in 1951 forbidding it, but the law is recent and not often enforced. There are small communities where polyandry is practised but these are numerically insignificant. The Christians cannot marry more than one wife in church but the customs of the country are often stronger than the law of the church and the law of the country, and some Christians do in fact live with several concubines.

Marriages in all communities are mostly arranged between the families rather than between the marriage partners as in European countries, and they take place at a very early age. Child marriages are still very common; such marriages are only consummated after the wife "comes of age". A number of widows have lost their husbands before this has happened.

The tables show that the highest admission rate in both sexes occurs amongst married persons, the figures being 1·12 per 100,000 of the population for men and 1·13 for women. Single men with 0·811 show a slightly higher rate than women with 0·221. Widows with 0·280 show a higher incidence than widowers with 0·108, but the numbers of admissions there are rather small and difficult to evaluate statistically. The status of widowhood is a

TABLE VIIA

Age Group	Popu- lation	Single		Married			Widowed		
		Number of Ad- missions	Admissions per 100,000 of the Age Group of the Total Population	Popu- lation	Ad- missions per 100,000	Admissions per 100,000	Popu- lation	Ad- missions per 100,000	Admissions per 100,000
5-14 ..	118,576	7	0.059	266	1	3.76	13	—	—
15-24 ..	72,753	118	1.62	11,893	22	1.85	341	—	—
25-34 ..	14,158	33	2.32	55,359	74	1.34	1,962	—	—
35-44 ..	2,268	9	3.96	51,560	57	1.11	3,433	1	0.292
45-54 ..	893	2	2.25	43,723	30	0.68	5,137	1	0.195
55-64 ..	389	—	—	17,199	21	1.22	5,065	—	—
65-74 ..	142	—	—	6,081	3	0.49	2,653	—	—
All ages from 5-74	209,179	169	0.811	186,081	108	1.12	18,604	2	0.108

The table shows the male population. The age distribution is derived from a 10 per cent. sample of the total population comprising 905,857 persons. The admissions are from Mysore State only.

TABLE VIIB

Age Group	Popu- lation	Single		Married			Widowed		
		Admis- sions	Admissions per 100,000 of the Age Group in the Population	Popu- lation	Admis- sions	Admissions per 100,000 of the Age Group in the Population	Popu- lation	Admis- sions	Admissions per 100,000 of the Age Group in the Population
5-14 ..	114,574	10	0.09	5,759	—	—	69	—	—
15-24 ..	13,498	15	1.11	62,856	59	0.94	2,762	—	—
25-34 ..	1,411	2	1.42	59,382	86	1.45	8,405	3	0.41
35-44 ..	500	1	2.00	32,867	36	1.09	13,547	7	0.52
45-54 ..	338	1	2.96	15,728	16	1.02	17,642	6	0.34
55-64 ..	210	—	—	4,610	9	1.95	14,749	2	0.14
65-74 ..	91	—	—	941	—	—	7,026	—	—
All ages from 5-74	130,622	29	0.221	182,143	206	1.13	64,200	18	0.28

This table shows the female population. The age distribution is derived from a 10 per cent. sample of the total population comprising 905,857 persons. The admissions are from Mysore State only.

distressing one. The widow never remarries (except in extremely rare cases) and she assumes a lowly place in the household where she considers herself as only tolerated. In the case of the widowers the situation is different. He can remarry. The fact that there are so few admissions amongst widows may mean that there is a lower incidence of mental illness due, perhaps to the absence of childbearing; it may also mean that since their place in society is such a low one they are not often referred for treatment or admission. Very young widowhood does not seem to bring about a high admission rate. We did not know the ages at which these patients had become widows, and cannot say therefore whether a large number of widows had lost their husbands early in life.

As regards married men the high figure in the youngest age bracket cannot be regarded as significant in view of the very small number of admissions, namely 1. It is however interesting that of all admissions in that age group so many are single, namely 7. The largest number of admissions amongst married and single men seems to occur in the 25 to 54 age groups. The incidence of admissions amongst the single men is very considerably higher in all these 3 age groups, particularly in the 35-44 age group, where it is 3.96 per 100,000 of the population as compared to 1.11 amongst the married men. The meaning of these differences is difficult to understand. Again it would be dangerous to equate admission numbers with incidence of mental illness as such. It is quite possible that singleness and mental instability are correlated, but other interpretations are also feasible.

It is interesting to note that amongst the married men a second peak of admission occurs in the 55-64 age group which is not seen amongst the

unmarried. In the single group the peak occurs in the 35-44 group, in the married group there are two peaks, one in the 15-24 and one in the 55-64 groups.

This trend is all the more interesting as it seems almost the same amongst the women. The single women are admitted in relatively larger numbers than the married women and more frequently in the 35-44 age group, whereas the married women show the two peaks, one in the 25-34 age group with 1.45 per 100,000 and again in the 55-64 group with 1.95 per 100,000. If a woman does not get married this carries a stronger stigma in India than in Europe and is worse for her than for an Indian man. It is possible that one of the reasons why these women did not get married was mental instability, or physical illness predisposing to mental illness. The second peak in the married groups may be due to a difference in the type of mental illnesses. If it means that there is a higher incidence of affective disorders in the married groups this would suggest a most interesting line of enquiry. Unfortunately, the diagnosis on the case-sheets did not seem an item which was sufficiently uniform to allow of reliable statistical analysis. As was pointed out above, the turnover of the hospital is very rapid. The patients are from several linguistic groups, and no single doctor can be expected to speak all these languages. The possibilities for special investigations were at that time limited, which in view of the high incidence of organic syndromes, impairs accurate diagnosis very much. Also the patients tend to leave the hospital as soon as the acute phase of the illness has passed, and there is little opportunity for prolonged observation.

The hospital has since been upgraded to the level of a teaching hospital with many more doctors, psychiatrically trained nurses and excellent laboratory facilities, biochemical, electro-physiological and psychological, which will make such an analysis possible in the future.

It is noteworthy that the maximum proportion of admissions for women occurs in the 25-34 group, although the childbearing age begins much earlier. Extremely early childbearing, such as would hardly ever occur in Europe but is frequent in India, does not seem to contribute much to admissions. It cannot be repeated often enough that this does not necessarily mean that puerperal mental illnesses do not occur more frequently. The position of the young married wife in society is different from that in the European set-up. The young wife moves into the house of her husband. There she is expected to assume a very modest place in her husband's household and is exposed to much friction. "Mother-in-law trouble" is liable to affect the young wife rather than the young husband as is the case in Europe or the U.S.A. For childbirth she usually returns to her mother's house. This situation may well prevent the young mother who is stricken by mental illness from being admitted to hospital.

The figures create more new problems than they answer and more research is required before an interpretation can be ventured.

SOCIAL AND CULTURAL FACTORS

Religion and Caste

The religious composition of India, and even more so the caste structure of the society, are unique in the world. Their existence could prove a gold-mine of information for comparative psychiatry. The religious and caste communities, although living in close proximity, provide widely differing social and cultural backgrounds for the individuals belonging to them. Furthermore they do not intermarry, and many communities sanction marriage between

uncle and niece, i.e. a girl being married to her mother's younger brother, or first cousin marriages, i.e. a girl being married to her maternal uncle's son or her paternal aunt's son (never to a paternal uncle's or maternal aunt's son). There seems to exist almost an experimental situation for psychiatric, genetic and social research. Considering that many families are polygamous the "experiment" is almost perfect. All the more surprising that this unique source of research information has not yet been tapped.

Unfortunately the information provided in the case notes under caste is not detailed enough to allow a satisfactory analysis. We had to restrict ourselves to very few items. Furthermore, the Indian government is anxious to root out the caste system and has excluded this item from its census reports. We therefore had to use the figures given in the previous Census of 1946. We were reassured that no major changes need be expected. A further complication is the caste system itself. The four main castes Brahmins (priests), Kshatrias (warriors), Vyshias (merchants) and Sudras (farmers) represent a crude oversimplification of the situation. Hutton (1951) points out that the origins of the caste system are multiple, with such diverse factors as migration, race, colour, language groups and many more playing a part. For instance a Brahmin from the Kanarese-speaking group will not marry a Brahmin from the Tamil-speaking group. There are even differences of caste between Tamil-speaking Brahmins from Madras, and Tamil-speaking Brahmins from Kerala. The situation in Mysore State is as complex as elsewhere in India. There are five major language groups represented in the country, and considering the influence of this factor alone on caste one may see the infinite complexity. Furthermore, the castes do not always agree as to their position in the hierarchy. To quote only one example from the Census Report (1941): The Vyshakarma caste are counted officially as Sudras, but they consider themselves Brahmins and have made representation with the Census Office to have their status amended. Another complication is provided by the Lingayats (non-Brahmin priests). They are a very numerous group in this state and are officially regarded as Sudras. But historically they are more a splinter religion or sect of Hinduism with their own internal caste system.

The other religions, although officially condemning the caste system, nevertheless show rather similar structure within their own flock. Even the Jews are divided into "white Jews" and "black Jews"; the former claim to have come to India in the second century, whereas the latter are said by the "white Jews" to be converts. They behave towards each other rather like castes.

As will be seen from this the situation is extremely complex and, unless the information is collected in the most circumspect and informed way, may easily lead to wrong conclusions. This becomes clear when the official, rather crude census results are examined in the light of far more detailed investigations of small communities or villages by anthropologists. Srinivas (1955) has reported on a village in Mysore State called Rampura. In this village of 1,523 inhabitants alone there are 17 castes as well as a Muslim community. Every one of these is separated from the others by endogamy, prohibition of contact through food (commensalism), occupational specialization, separate caste courts, etc.

Keeping all these limitations in mind we have attempted to analyse whatever information is available both from the case records, and from social and anthropological research in the population.

Table VIII shows the main religious communities and castes both in the population and amongst first admissions to the hospital. The Hindus with

TABLE VIII

Communities				Number of Persons	Number of First Admissions	First Admissions per 100,000 of the Population
Muslims	..	..	..	485,230	64	13.2
Indian Christians	..	..	..	98,580	31	31.5
Other Christians	..	..	..	14,273	3	21.6
Parsees	..	..	..	401	—	—
Jains	..	..	..	32,858	4	12.3
Brahmins	..	..	..	295,466	124	42.0
Kshatrias (Warrior caste)	..	..	..	45,274	7	15.5
Vyshias (Merchant caste)	..	..	..	55,811	31	55.5
Sudras (Workers caste)	..	..	..	4,869,824	232	4.7
Scheduled castes* (Untouchables)	..	..	..	1,405,067	78	6.9
Other Hindus (not specified)	..	..	..	14,188	58	Not applicable
All Hindus	..	..	..	6,686,630	530	7.9

* These include: A.K., Banajara, Vodda, Koracha, Korama.

This table shows that the Vyshias, Brahmins and Indian Christians have the highest admission rates. The Parsees and "other Christians" (mostly Europeans or Eurasians) are probably not strictly comparable with the other groups in this respect.

6,686,630 are by far the largest group, and amongst them the Sudras and scheduled castes outnumber by far the high castes. As regards admission rates to hospital the Hindus as a whole have the lowest rate, that is not counting the small community of Parsees who are in a rather special position. Within the Hindus however there are enormous differences. The Vyshias show the highest admission rates of 55.5 per 100,000 of the population, the Brahmins come second with 42.0.

The next largest rate are the Indian Christians with 31.5. It must not be assumed that the Christians are largely converts. Many of them are descended from converts of several generations ago. The group "other Christians" are mainly Anglo-Indians and a small group of Europeans. Their rate of 21.6 is next in size. Muslims with 13.2 and Jains, a Hindu splinter religion originating in the twelfth century, with 12.3 are rather similar. The last group "other Hindus" cannot be compared. The admissions classed under this heading fall into this group because of unprecise entries on the case sheets. In the census it includes Hindus not falling into any of the provided major categories.

It is most difficult to interpret these findings. The important question which arises of course is whether the admission rates bear any relationship at all to the incidence of mental illness. This question cannot be answered without further research. Some light may be thrown on these figures from the findings reported in the next section.

LITERACY

Literacy amongst the population has been assessed by the Census Report (1951, p. 161). Unfortunately no such enquiry was made amongst the patients admitted to the hospital. In order to correlate literacy with admission rate to hospital we have to use an oblique approach. We have therefore examined the correlation between literacy and communities. Literacy was defined in the Census Report as "a person's ability to read and write a letter". No further details are noted.

Figure 3 shows that there is a certain similarity between the admission rates in the various communities and the literacy. The Vyshias have a larger

admission rate with 55.5 than the Brahmins with 42.0, although literacy is higher amongst Brahmins with 59.4 per 100 as compared to 43.9 amongst Vyshias. It is possible that the influence of literacy is offset by the fact that Vyshias as merchants tend to live very much in the cities. Parsees and "other Christians" have not been included as they are in rather a special position.

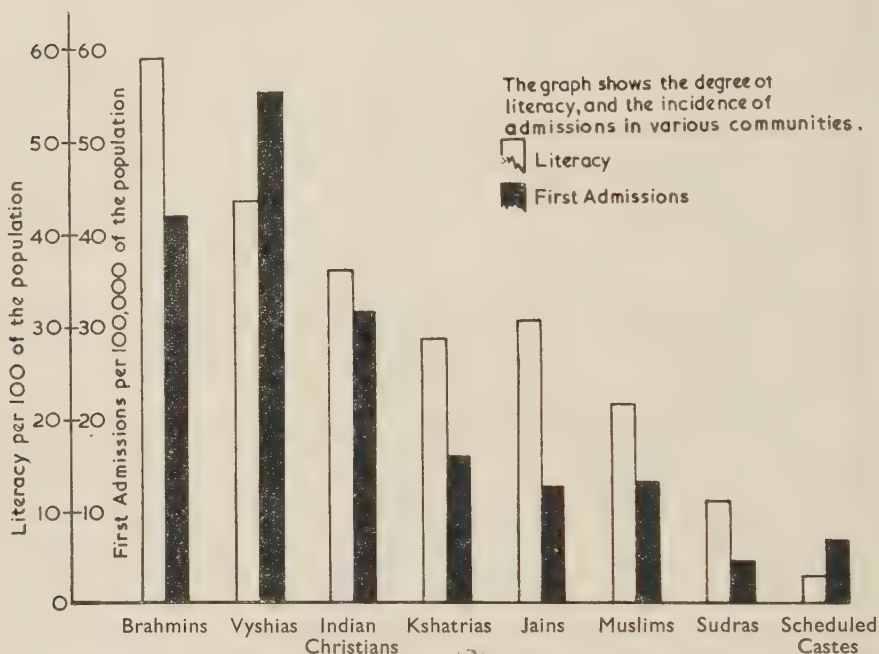


FIG. 3

The degree of literacy amongst them is quite out of proportion to the other communities. Also they are liable to use other facilities in case of mental illness than the state mental hospital. They most probably turn to private institutions outside Mysore State. Amongst the other communities the relationship is commensurate, except amongst the scheduled castes where both admission rates and literacy rates are rather small. From this figure it would appear that literacy would affect admission rates to hospital to a certain extent. To be able to use such facilities as hospitalization, familiarity with the services available and also a certain trust in these would seem necessary. Literacy would both make the available information accessible and also indicate a certain attitude in the community which is less parochial and ready to avail itself of services provided by the state.

CONCLUSIONS

These figures, interesting though they are, pose more questions than they answer. They clearly point to the possibilities and the necessity of further research. It would be interesting to know to what extent the hospital admission figures are related to the actual incidence of mental illness. It would be foolish to expect the incidence figures to be anything as exact as admission figures. The incidence of mental illness very much depends on the definition of this term. The admission figures to mental hospitals in England and Wales have vastly

increased over the past 36 years. In 1920 they were 21,703 (Royal Commission 1954) as compared with 50,405 in 1956 (Annual Report, 1956). Everyone will agree that this is largely due to an alteration in the service which the medical and psychiatric organizations in this country are rendering to the community. It would be rash to conclude that it reflects an increase of mental illness in the community. The facts are much more elusive than that. The psychiatrist may be expected to deal with problems which were formerly regarded as belonging to the domain of the clergy, the law courts and the police, the educationist, etc., and is therefore dealing with different groups of people than he used to.

This is certainly true for India. One would expect the admission figures and incidence figures in the U.K. to have a closer relationship than in India due to a wider development of mental health services.

Hare (1956) when investigating the incidence of certain mental illnesses in Bristol believes that few schizophrenic patients will escape admission to a mental hospital at one stage or another of their illness. Lemert (1951) points out that this may be far too optimistic a view. It would certainly be too optimistic for Mysore State. A visit to a village near Bangalore in 1956 showed us that, if indeed proof were needed. This is a village with about 900 inhabitants and not more than 5 miles from the mental hospital. It is partly an industrial village, as it has a large textile mill. Both the distance from hospital and industrialization would lead one to expect a closer link between admission and incidence figures. We asked the Shambog (the village clerk) if he knew of any mental cases in his village. He was most helpful and not only mentioned a few but actually sent for four such persons whom he knew. We found that they were all young persons, perhaps 20 years or so. Two had what appeared to be chronic schizophrenia. One was a farmhand, the other a shepherd. One person was a mental defective, perhaps of imbecile level. We were unable to come to any conclusions regarding the aetiology. The fourth person had a basal ganglia disorder which was said to have been present since childhood. He had choreo-athetotic movements and appeared mentally defective. He was however working in a reduced capacity. None of these four had ever been to the mental hospital either as in- or out-patients. We have no doubt that the state of affairs in this village is typical for the rest of the country.

So much for gross mental illnesses. The incidence of other and lesser forms is even more difficult to assess. Certain disorders, which would not necessarily be regarded as "psychiatric" in Europe would be considered to be due to "possession" by spirits, and treated in the villages by the village priest, sorcerers, etc. The most notable disorder of this group are all forms of infertility in women. The villages have a very elaborate organization governed and maintained by tradition to deal with problems which Europeans would consider to be related to health. Very little is known about this. An American anthropologist Woodruff (1956) investigated this problem in a community of Pariahs in Bangalore and reported on it in a lecture at the All India Institute of Mental Health. She found the following: A person in the community who by tradition holds such office would see anyone who had a complaint regarding his "health". The distinction between physical and mental ill health is not sharp and strikingly "modern" in that respect. These "patients" would be given treatment by him or referred to see either a practitioner of the indigenous schools of medicine—ayurvedic or unani—or to a "Western" trained doctor. The local treatment might be anything from an application of herbs to a sacrifice of a lizard or a cock. Sometimes an exorcism would be arranged. There is no reason to believe that these treatments are less effective than certain

types of psychotherapy or than the prescription of virtually placebo "bottles" in a European consulting room, and they may be dealing with large numbers of minor forms of mental ill health. It would indeed be very difficult to give a meaningful definition of mental ill health by which to assess the incidence. It would almost certainly be misleading to simply transfer European or American definitions to conditions in Mysore State, which shows such enormous differences in cultural and social patterns.

As regards admissions to mental hospital it is obvious that Mysore State is undergoing a change in this respect. The admission figures have grown enormously in the last ten years and show every sign of further increases with the growth gathering momentum.

Some factors which correlate with admission rates are not unexpected: distance of residence from the hospital is an obvious factor, particularly if one considers the enormous distances involved. Dwelling in large towns is also a factor, which overrides the distance. The admission rates from very distant large towns are higher than those from very near rural districts. These factors will obviously have to be taken into account in organizing the mental health service in Mysore State (Hoenig *et al.*, 1956). It is perhaps surprising that certain other social factors did not seem to affect the admission rates. Notable are overcrowding, population-density and size of township, provided it was below the size of the very large cities. It must be remembered that the admission figures are relatively small at present and these factors may come to play a part in the future.

Sex and marital status certainly correlate with admission rates. In the case of sex the probability is that the male breadwinner is brought for treatment more readily than the female, and also that home nursing of an acutely disturbed male patient may be more difficult than a female one. In the case of marital status the causal connection is less clear. Married persons show a higher admission rate than single or widowed persons. The negative finding amongst widows is very surprising here, considering their distressing social status. It is possible that because of their social position abnormal behaviour is more readily accepted by the community and treatment not sought so regularly. There is no evidence that very early childbearing is a contributory factor in admission rates. Single persons, particularly men, show a higher admission rate than single women, particularly in the younger age groups. The significance of this is impossible to assess reliably without further investigations.

As regards age there are certain very interesting correlations, particularly the high incidence amongst the younger age groups. Another point of interest is the second peak amongst the married persons in the higher age brackets. It was pointed out however how precarious a piece of information age is, amongst the patients as well as in the general population. Conclusions here must therefore remain very tentative indeed.

A most interesting finding is that caste or religious community shows correlation with admission rates, with Brahmins, Vyshias and Indian Christians showing a much higher rate than the others. Various possible explanations must be considered. Most communities are in-breeding often in a genetically highly significant degree, i.e. by uncle-niece, or first cousin marriages. It may well be that this increases the incidence of mental illness in certain communities where the stock predisposes to this. It is however possible that the above communities are more numerous in the large towns, or more aware of the hospital services, modern medicine, etc. and may therefore use them more.

These communities are the most literate ones and this finding would lend support to such a theory. The finding would suggest further possible lines of research. Here again conclusions must be very guarded in view of the limited amount of available information.

SUMMARY

The admission figures of the Mental Hospital in Bangalore, Mysore State, are presented and analysed according to certain social, geographical and cultural factors. These are correlated with the general population of Mysore State.

Admission to the mental hospital is higher:

1. In large cities, than in smaller towns or rural communities.
2. In districts situated nearer to the hospital, than in those further away.
3. Amongst men, than amongst women.
4. Amongst married, than amongst single or widowed persons.
5. Amongst the younger age groups, than amongst the older ones.
6. Amongst certain communities, notably the Brahmins, Vyshias and Indian Christians.

The difficulties of assessing the possible significance of these findings are discussed.

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ABNORMAL AND PERSONALITY CORRELATES OF CERTAINTY

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INTRODUCTION

IN the present investigation verbal statements of certainty, or confidence in the performance of immediate recall and recognition, were related to the neurotic/schizophrenic dichotomy and, within the neurotic group, to personality questionnaires of rigidity, extreme response set, extraversion and neuroticism. The aim was to find pointers to conditions favourable for the investigation of individual differences in certainty and to assess the relative validity of a number of independent personality criteria. Statements of certainty analysed are derived from an immediate recall experiment, the results of which have already been reported (6). An analysis of the interaction between certainty and other measures derived from the same experiment is to be presented at a later date.

Greatest emphasis is on the conditions under which certainty varies. If desire for certainty, as is frequently maintained, falls under the wider concept of motivation, complex relationships not uncommon in motivational research may emerge. Apart from a number of other factors (24, 27), difficulty level (31) and insight in the situation to be judged (36) are already known to play a part. The resulting significant interaction between such factors may explain the failure to find a substantial general factor of certainty (38) and the dependency of intercorrelations on external conditions (20, 27).

Strong emphasis is also placed on the problem of independent criteria for certainty. In the motivational field such standardized criteria are lacking. Hoping to close this gap, the author has resorted to a particular type of "rigidity" scale, expressing strong desire for achievement, and an extreme response set check list, referred to later. Other scales have been used to investigate drive characteristics. Three of these are the known scales of Manifest Anxiety (MAS) by Taylor and the Extraversion and Neuroticism scales by Eysenck (21). Judging by their content, however, none of these scales can be said to express motivational intensity directly. The necessity to introduce corresponding *ad hoc* hypotheses certainly decreases the stringency of the experiments performed.

In search for empirical predictions, the difficulty is that *certainty* has been little considered from the personality point of view. Moreover, its relationships to other similar fields like level of aspiration, need for security, self-esteem, ego-involvement, a.o., are practically unknown. It was thought that by creating an easy manipulable and effective tool for measuring certainty in both individual and group situations and by developing standardized independent criteria, much could be done to facilitate future research. The present paper lays the foundation to these aims.

The most relevant literature is now reviewed, separately for the test scores used.

From negative correlations between Thurstone's neurotic inventory and confidence, as derived from performance tests (31), and from the reported "negative self attitudes of neurotics" in TAT stories (18), one might expect neurotics as a group and normals with neurotic tendencies to suffer from low confidence. As, however, neuroticism is known to correlate highly with the MAS (17), results by Weiner and Ross (37) appear to contradict the above statement. Working with undergraduates, these authors found the high anxiety group to be more confident in a line judgment experiment than the low anxiety group. Similarly, Wolff (38) found certainty in learning to be positively related to MAS. One further important variable may be that of extraversion. As has been shown in level of aspiration experiments, judgment of positive confidence in test performance is significantly higher in the reputedly extraverted hysteric than in the introverted dysthymic (10, 13).

About certainty in schizophrenics little direct knowledge has been presented. Schizophrenics have, however, been found to have positive attitudes toward the self (18), to increase their self-esteem in the type of self-recognition experiment developed by Wolff (9), and to be overly optimistic in level of aspiration experiments (25, 29).

Warranted certainty or, as called in the present experiment, *adequacy of certainty*, has not received much attention from the personality point of view, although its importance increased through the knowledge that confidence of judgment is by no means equivalent to correctness of judgment (26). Although over-confident persons are, more frequently, less correct than others, the result has been obtained that the most confident were also the most accurate (7). This, again points to the importance of studying the conditions under which such reversals take place. Block and Petersen (5) noted that "despite expressions of confidence in all his judgments, the overly confident individual is seen by the staff as significantly less self-reliant and independent in judgment than the person with warranted confidence". When comparing neurotics with schizophrenics, the "self-enhancing defences" of the paranoid schizophrenic were stated to be "based on unrealistic self-appraisal", whereas the neurotic negative self attitudes were based "on a realistic perception of disturbances within the self" (18). Within neurotics, judgment of performance in level of aspiration tasks was consistently more realistic in hysterics than in dysthymics (10, 13, 29).

Certainty is scored in terms of the following grades: +2, +1, 0, -1, -2. Judgments of +2 and -2 are treated as *extreme responses*. A certain amount of generality of this test taking attitude has been found. Rundquist (34) obtained a correlation of 0.40 ($N=111$) between "two situations, one immediately following the other". Berg's (3) correlations between his Picture Reaction Test and Word Reaction Test varied between 0.34 and 0.77 (N ranging from 22 to 95).

In the present context, the main interest is not in generality of this kind. It is, of course, of great interest to know the magnitude and structure of inter-correlations. This problem is, however, secondary to and dependent upon the knowledge of the conditions under which magnitude increases or decreases and the structure changes. In this respect, Cronbach's (8) formulation of one such condition appears relevant, that response sets become more influential as test items become difficult or "ambiguous".

Extreme response as such is a complex phenomenon and the literature to date indicates that *positive extreme response* generally "matters more" in validation. Allport and Hartman (1) have shown that persons with extreme opinions are also more positively confident. When attributing positive and

negative characteristics to photographs of persons, neurotics were far more negative than psychotics (11). Using Berg's (4) Perceptual Reaction Test, Barnes (2) found that psychotics, in particular schizophrenics, scored high on extreme responses. This result was mainly achieved via the positive extremes. Relating the same test to MAS, Lewis and Taylor (28) observed that the high anxious choose more extreme responses than the low anxious subjects. This is in accordance with the higher positive certainty of anxious persons already quoted. For these reasons, response set will be treated separately for positive and negative extremes.

The last score, to be called "*subjective recognition*", is of incidental importance, as it was mainly used to derive the adequacy of certainty score. After a recall test of 10 FRT patterns (see Method section) the subject is asked to recognize them from a much greater number of very similar patterns. Scores higher than 10 would indicate that subject "over-includes", i.e. he recognizes more patterns than warranted. On this score, schizophrenics should score higher than neurotics. The relevant literature is extensively reviewed by Payne (32).

The first part of the analysis deals with differences between neurotics and schizophrenics, the second part with correlations between certainty test scores and questionnaire criteria. The empirically derived predictions are shown in Table I.

TABLE I
Overview of General Expectations

Direction of Predictions			Schizo- phrenics over Neurotics	High over Low Extra- version	High over Low Neuroti- cism	High over Low Rigidity	Positive q- extremes	Negative q- extremes
Certainty	..	..	+	+	?	+	+	-
Positive c-extremes	..	..	+	+	?	+	+	?
Negative c-extremes	..	..	-	-	?	-	-	+
Subjective recognition			+	?	?	+	+	?
Adequacy of certainty			-	+	?	-	-	?

c=certainty extremes; q=questionnaire extremes (Soueiff Check List).

The general expectations for extraversion appear clear from the literature. Because of conflicting results no predictions were made for neuroticism. As regards "rigidity", the author makes no attempt to defend this badly criticized term. Although the originator of the rigidity scale (see Appendix) has, by factor analysing a number of rigidity and related scales, found a general factor of rigidity, this scale was mainly selected for reason of its verbal rather than factorial content. Most of the items deal with preference for concentrated work, will-power, tenacity to strive for high goals and emphasis on certain extreme attitudes. These characteristics appear to reflect high intensity of one or another form of drive, or motivation, and are for that reason expected to correlate positively with the desire for certainty.

Finally, positive and negative extreme responses from Soueiff's "Personal Friend" Check List, described in the Method section, were chosen as independent criteria. On account of the already discussed positive nature of extreme response, certainty may well correlate significantly with positive extreme response. Whether negative extreme response correlates positively or negatively with certainty is more difficult to decide. In this respect the tentative hypothesis

is made that the direction of correlations follows the signs of response attitudes, i.e. negative certainty correlates positively with negative extreme response and vice versa.

In accordance with other authors (19, 33, 16) who consider schizophrenics to be more rigid than neurotics, the general hypothesis is used that the correlations between certainty and rigidity follow the pattern of discrimination between neurotics and schizophrenics. The dimensional hypothesis is accepted that certainty increases in the order: low rigid—medium rigid—high rigid neurotics—schizophrenics.

The predictions stated in Table I are of a general kind, as there is no adequate empirical evidence to specify the exact conditions under which they bear out. The main aim of the present analysis is then to establish more specific predictions.

METHOD

1. *Tests.* As this is an extended report of an experiment published elsewhere (6), some of the experimental conditions are briefly stated. Of the two tests used under various conditions, one had already been described in detail in the above-mentioned paper. This test, the *immediate recall* version of the Figure Reconstruction Test (FRT) requires drawn reproductions of patterns of five geometrical shapes each. Ten such patterns form one test set and the distribution of all test shapes of a set is shown in Figure 1 (top left). To the bottom left of this figure an example of a reproduced pattern may be seen complete with statements of certainty concerning the correctness of reproduction attached to each individual shape.

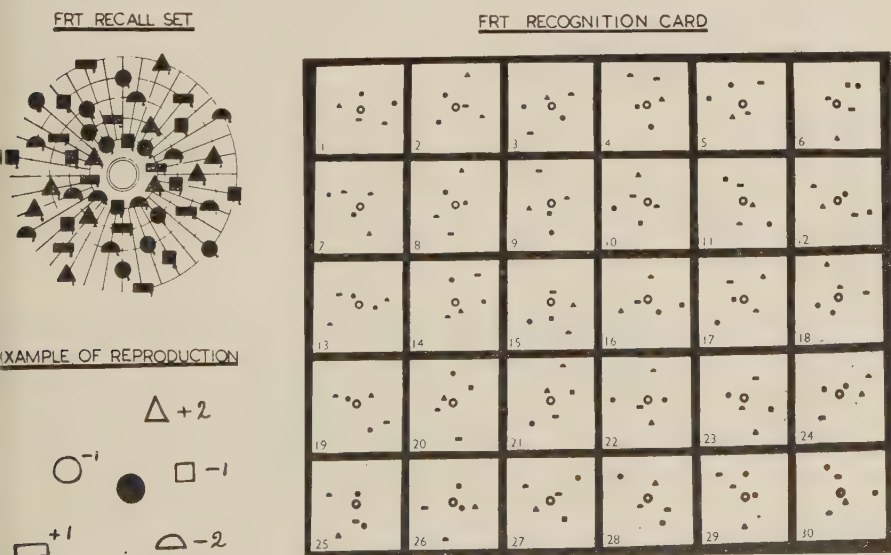


FIG. 1.—The Figure Reconstruction Test (FRT). *Top left:* Distribution of a set of 10 patterns of five shapes each. *Bottom left:* Example of a reproduction complete with statements of certainty. *Right:* Recognition card with 30 patterns, 10 of which being the actual test patterns.

The right part of Figure 1 is occupied by the *FRT recognition* card, by means of which the second test may be described. This card contains 30 FRT patterns, 10 of which actually form an FRT recall set, the remaining 20 being

dummy patterns. After a subject completed an FRT recall test, the respective 10 patterns were mixed up with 20 similar ones in a pre-arranged random order. The latter patterns had never been seen by the subject before. The entire recognition set of 30 patterns was then shown twice in succession, first with an exposure time of two seconds and then with a continuous, or unlimited, exposure time. The subject's response consisted of stating whether he thought he had seen the pattern during the preceding recall period (subjective recognition) and of stating the degree to which he was certain that this recognition response was correct. Subsequent to the present experiment the method has been greatly facilitated. Individual presentation of the 30 patterns is now discarded and the subject is given a recognition card, shown in Figure 1, together with an appropriate response sheet. This method permits group administration, which appears justified by the fact that the two seconds and continuous exposure conditions for recognition did not materially affect any of the results concerned.

2. *Scores.* As the example shown in Figure 1 (bottom left) demonstrates, *recall* consisted of drawing the five test shapes from memory around a central reference point, which is for scoring purposes only. The subject is requested to add, according to his feeling of correctness of reproduction, one of the following degrees of certainty to every one of the shapes reproduced:

- +2 = very certain
- +1 = somewhat certain
- 1 = somewhat uncertain
- 2 = very uncertain

No other comment was made, nor was the subject instructed as to how well he did until the test was completed.

With ten patterns of five shapes each, 50 certainty statements per test set are obtained. The four certainty grades are conveniently displayed in front of the subject so as to facilitate response. The same applies to the actual test shapes themselves.

During *recognition* the same procedure to obtain certainty responses is used. As stated in the preceding point, subjects respond first to each of the 30 patterns with either "yes" or "no". These two responses, which are termed "subjective recognition", provide, in combination with four certainty grades, the following eight response types, which are recorded by the experimenter.

Yes (subjectively recognized)	+2=I am very certain I saw this pattern during the preceding recall test.
	+1=somewhat certain it was there.
	-1=it was there but I am somewhat uncertain.
	-2=I think it was there but I am very uncertain.
No (subjectively not recognized)	+2=Very certain it was not there.
	+1=somewhat certain it was not there.
	-1=I don't think it was there but I am somewhat uncertain.
	-2=I don't think it was there but I am very uncertain.

The description is now complete to define the test scores used as follows:

Degree of certainty (recall): mean degree of certainty per shape, regarding signs and weights of 1 and 2. All computations are worked out to two decimal places, the range being +200 to -200. Total number of responses in one test set is 50.

Degree of certainty (recognition): mean degree of certainty per response, otherwise as above. Total number of responses per test set is 60, 30 for each, the two seconds and "continuous" exposure conditions.

Extreme positive response set: Extreme certainty responses, hereafter c-extremes as against q-, or questionnaire extremes, are scored separately for positive and negative set. The score for positive c-extremes is the sum of +2 (very certain) responses. As the total of all responses is 50 for recall and 60 for recognition, individual scores of the latter are adjusted to 50 so as to enable direct comparison. Range is from 0 to 50.

Extreme negative response set: sum of -2 (very uncertain) responses, otherwise as above.

Subjective recognition: frequency of subject responding with "yes" during recognition or number of subjectively recognized patterns. Range is from 0 to 30, as the number of responses is 30 for any one recognition exposure condition.

Adequacy of certainty (recognition): mean degree of certainty for all correct responses minus mean degree of certainty for all wrong responses, signs regarded. This was done separately for all "yes" and "no" answers obtained when testing for subjective recognition. Each of the two recognition conditions (two seconds and continuous exposure) provided 30 responses, separately for the two recall conditions of 30 and 2 seconds exposure. This provides a total of 120 responses per test day on which the adequacy score is based as follows.

<i>"Yes" Responses</i> (subjectively recognized)					<i>Sum</i> <i>Certainty</i>	<i>Mean</i> <i>Certainty</i>	<i>Adequacy</i> <i>of</i> <i>Certainty</i>
				<i>f Responses</i>			
Correct	..	..	..	32	+48	+150	—
Wrong	..	..	..	47	+71	+151	-1
<i>"No" Responses</i> (subjectively not recognized)							
Correct	..	..	..	33	+31	+94	—
Wrong	..	..	..	8	+3	+38	+56
					—		
					120	score=	+55

3. *Design*. A sufficient number of parallel tests were constructed so that no test was given twice to a subject. There were two sessions conducted on consecutive days with subjects tested individually. Each session consisted of two recall tests, one of 30 and one of 2 seconds exposure. Recognition was tested after each of these tests. The order of the recall exposure conditions was balanced within and between the subjects so as to minimize sequence effects. The respective basic design was as follows:

	Recall	Recognition	Recall	Recognition
First day ..	30 seconds	2 seconds— continuous	2 seconds	2 seconds— continuous
Second day ..	2 seconds	2 seconds— continuous	30 seconds	2 seconds— continuous

With the exception of adequacy of certainty scores are derived separately for the various conditions used as indicated along the abscissa of Figures 2 and 3.

For adequacy a total score was employed separately for tests 1 and 2, as shown in Table VII. This was done because, under the present scoring conditions, high unreliability was observed for scores derived from the individual test conditions. This unreliability is apparently due to large individual variation in subjective recognition, which was used as a basis for scoring. At the expense of subjective recognition, the reliability of adequacy may be considerably raised by standardizing the number of "yes" and "no" answers in recognition.

4. *Questionnaire criteria.* Certainty test scores are correlated with the following questionnaire criteria:

Extraversion: 24 items (MPI, 12)

Neuroticism: 24 items (MPI)

Rigidity: 28 items (30)

Positive q-extremes: extreme positive response set (+2), derived from the questionnaire described below.

Negative q-extremes: extreme negative response set (−2).

These questionnaire extreme responses (q-extremes) are derived from an extended version of Souciff's "personal friend" check list (35). Subjects indicate how much they like or dislike their personal friends to have certain characteristics by ticking off 101 adjectives and using the following grades: +2, +1, −1, and −2. Extreme response set is simply scored as the number of +2 and −2 responses.

Intercorrelations between these scores, based on 48 neurotics, are given in Table II.

TABLE II
Intercorrelations of Questionnaire Criteria

	N	NR	+2	−2
Extraversion	−0·38†	−0·22	0·02	−0·06
Neuroticism	—	0·09	0·09	0·01
Nigniewitzky rigidity	—	—	0·40†	0·14
Extreme positive response set	—	—	—	0·42†
Extreme negative response set	—	—	—	—

N=48 neurotics. Significance: * = 5 per cent., † = 1 per cent. level

As the correlations are obtained from an abnormal sample, the significant negative correlation between extraversion and neuroticism is as expected (23). Rigidity correlates significantly with positive q-extremes, as expected, but not with any of the other criteria. The relatively high correlation between positive and negative q-extremes deserves further attention.

5. *Samples.* The four samples used were:

- 24 male neurotics
- 24 female neurotics
- 12 male schizophrenics
- 12 female schizophrenics

A detailed description of these groups has been given elsewhere (6). Age ranged from 18 to 48 years, with small differences between the groups. Physical treatment, signs of brain damage and, in the schizophrenics groups, symptoms of depression constituted criteria of non-selection.

RESULTS

Where applicable, results were treated by means of analysis of variance. Most of the findings are also presented graphically and the means pertaining to them are listed in tables. When examining the analysis of variance tables, the use of the following procedure should be noted. All interactions were first tested for significance against the original residual 1. Where significant interactions occurred the sums of squares of all interactions were added to that of residual 1. This results in residual 2 against which the significance of the variables employed was finally assessed. Omitting the insignificant interactions, the significant ones were listed separately.

1. *Degree of certainty.* Results for degree of certainty are shown in Figure (upper part) and in Tables III and IV.

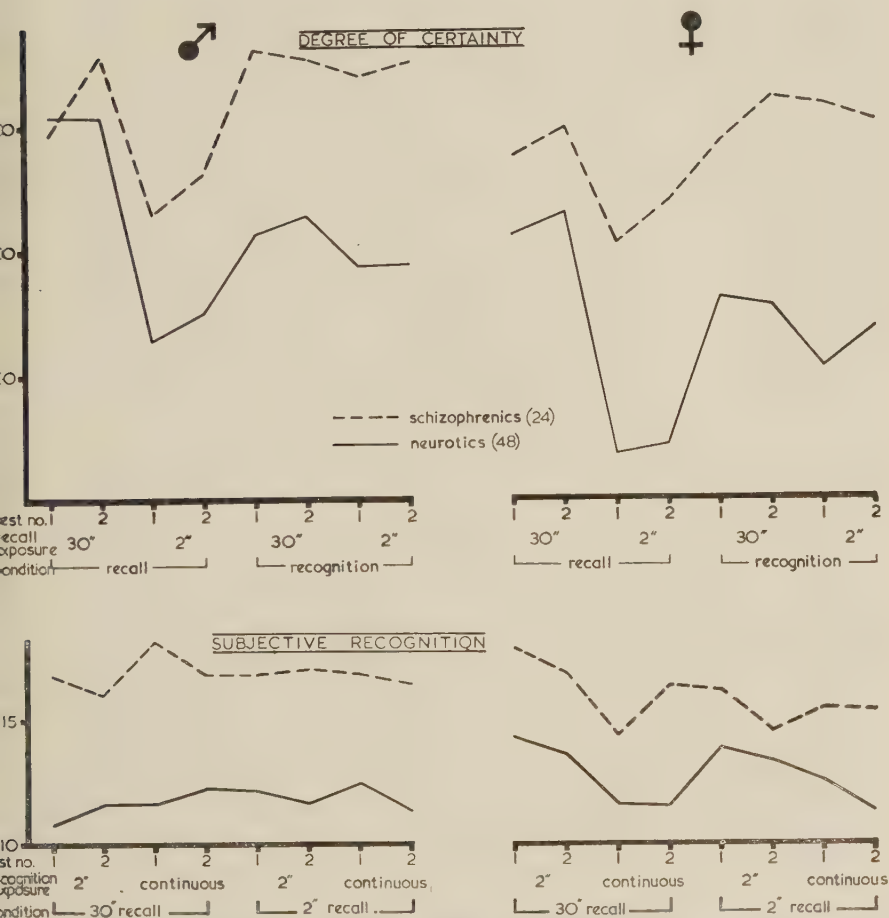


FIG. 2.—Upper part: Degree of certainty as related to neurosis and schizophrenia. Lower part: Subjective recognition as related to neurosis and schizophrenia.

Judging by Figure 2 and Table III, degree of certainty is almost consistently higher for schizophrenics than for neurotics. The analysis of variance (Table IV) reveals these differences to be highly significant. Means are stable,

TABLE III

Means of Degree of Certainty, Positive and Negative Extremes, Pertaining to Figures 2 and 3

Condition		Recall				Recognition			
		30 Seconds		2 Seconds		30 Seconds		2 Seconds	
		1	2	1	2	1	2	1	2
Degree of Certainty	mN	153.6	153.3	64.1	75.1	106.0	113.8	93.8	94.4
	fN	106.1	115.0	18.0	22.0	80.2	77.2	52.5	68.5
	mS	146.8	177.8	114.7	131.0	180.2	176.3	169.4	175.1
	fS	137.0	148.2	102.2	118.8	142.3	160.2	157.3	150.9
Positive Extremes	mN	38.3	38.0	26.4	26.6	27.9	27.9	25.8	26.0
	fN	31.4	33.8	19.6	20.1	20.4	20.6	17.2	17.4
	mS	38.7	43.4	31.3	34.7	43.3	42.3	40.3	41.5
	fS	36.0	37.3	27.6	31.8	33.3	38.8	38.0	36.5
Negative Extremes	mN	1.5	1.9	9.5	6.7	2.0	1.5	3.8	2.6
	fN	6.0	5.3	17.1	15.5	2.8	3.9	4.3	3.8
	mS	3.3	0.7	3.8	2.2	0.5	0.3	0.5	0.4
	fS	3.3	2.6	4.9	3.3	0.6	0.1	0.2	0.1

m=male; f=female; N=neurotics; S=schizophrenics.

TABLE IV

Analysis of Variance for Degree of Certainty and Subjective Recognition, Pertaining to Figure 2

Source	d.f.	Sums of Squares	Mean Squares	F-ratio
<i>Degree of Certainty</i>				
Tests	1	7,965.562	—	N.S.
Groups	1	494,432.253	—	150.061†
Exposures ..	1	241,572.249	—	73.317‡
Conditions ..	1	9,328.340	—	N.S.
Sex	1	153,468.061	—	46.578‡
Residual 2 ..	570	1,878,084.028	3,294.884	—
Total	575	2,784,850.493	—	—
<i>Significant Interactions</i>				
G×E	1	35,778.124	—	11.991‡
G×C	1	32,746.670	—	10.975‡
G×S	1	12,800.000	—	4.290*
E×C	1	122,033.777	—	40.901‡
Residual 1 ..	560	1,670,843.644	2,983.649	—
<i>Subjective Recognition</i>				
Tests	1	18.777	—	N.S.
Groups	1	2,153.320	—	10.746†
Exposures ..	1	62.673	—	N.S.
Conditions ..	1	1.360	—	N.S.
Sex	1	17.360	—	N.S.
Total interactions	10	337.860	—	—
Residual	560	112,215.050	200.384	—
Total	575	114,806.400	—	—

Significance: * = 5 per cent.; † = 1 per cent.; ‡ = 0.1 per cent.

as no significant differences are obtained between tests 1 and 2. Certainty varies significantly as a function of the exposure time which, judging by the means and the E×C interaction, is largely due to the recall condition. Most important is, however, the significant interaction between G×E and G×C, showing that the differentiation between neurotics and schizophrenics is highly dependent on the experimental conditions stated. This fact is amply demonstrated in Table XII further below. Group differences are not significant for

any of the 30 seconds recall conditions. They increase in significance through 22 seconds recall to recognition.

2. *Extreme response set.* Extreme responses of certainty are presented in Figure 3, jointly and separately for positive (+2) and negative (-2) extremes. Means for these two subscores are given in Table III, with the analysis of variance added in Table V.

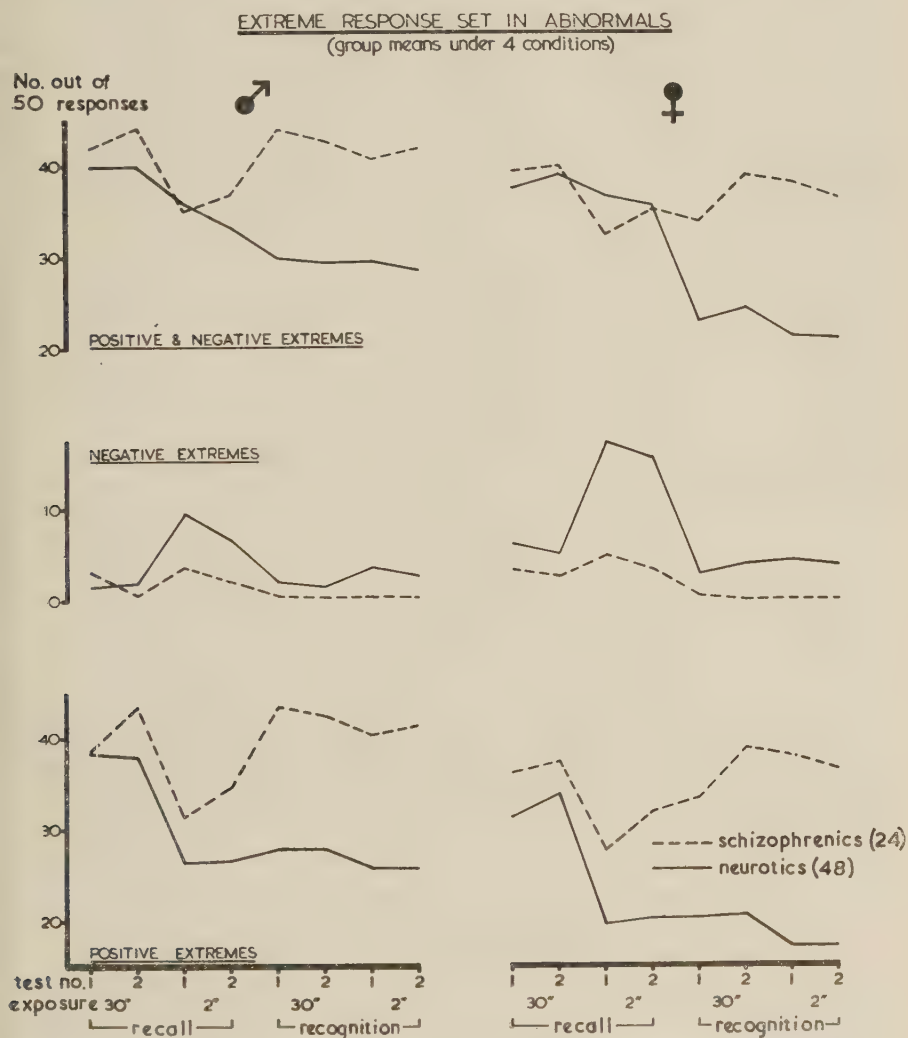


FIG. 3.—Extreme certainty response set as related to neurosis and schizophrenia.

Figure 3 clearly shows that fundamental differences exist with regard to positive and negative extremes. Schizophrenics scored constantly higher on positive extremes and neurotics almost constantly higher on negative extremes. This accords with expectation. Turning to the significance of these results (Table V), it is again found that retest had no appreciable influence. All other variables yielded highly significant F-ratios demonstrating the high degree of

TABLE V

Analysis of Variance for Positive and Negative Extreme Statements of Certainty, Pertaining to Figure 3

Source	d.f.	Sums of Squares	Mean Squares	F-ratio
<i>Positive Extremes</i>				
Tests	1	151.085	—	N.S.
Groups	1	15,716.167	—	122.331‡
Exposures ..	1	5,593.793	—	43.541‡
Conditions ..	1	1,181.640	—	9.198†
Sex	1	5,568.891	—	43.468‡
Residual 2 ..	570	73,229.422	128.473	—
Total	575	101,440.998	—	—
<i>Significant Interactions</i>				
G × C	1	3,559.571	—	30.196‡
E × C	1	2,786.961	—	23.642‡
Residual 1 ..	560	66,013.293	117.881	—
<i>Negative Extremes</i>				
Tests	1	92.641	—	N.S.
Groups	1	1,893.688	—	55.152‡
Exposures ..	1	1,636.877	—	47.672‡
Conditions ..	1	2,446.127	—	71.241‡
Sex	1	963.460	—	28.060‡
Residual 2 ..	570	19,571.497	34.336	—
Total	575	26,604.290	—	—
<i>Significant Interactions</i>				
G × E	1	593.689	—	19.721‡
G × C	1	152.397	—	5.062*
G × S	1	332.730	—	11.052‡
E × C	1	1,016.015	—	33.749‡
C × S	1	482.293	—	16.020‡
Residual 1 ..	560	16,858.606	30.105	—

Significance: * = 5 per cent.; † = 1 per cent.; ‡ = 0.1 per cent.

dependency of extreme response set on a variety of factors. For positive extremes between group differences were again highest. The remaining picture resembles that of degree of certainty closely. In the case of negative certainty, extremes scores are low and differ little for all but one condition, that of 2 seconds recall.

3. *Subjective recognition.* Results are demonstrated in Figure 2 (lower part) and Tables IV and VI.

TABLE VI

Means of Subjective Recognition, Pertaining to Figure 2

Condition (Recall Exposure) ..		30 Seconds Recall				2 Seconds Recall			
Recognition Exposure		2 Seconds		Continuous		2 Seconds		Continuous	
Test	..	1	2	1	2	1	2	1	2
Male neurotics ..	10.8	11.6	11.6	12.2	12.1	11.6	12.4	11.3	
Female neurotics ..	14.3	13.6	11.6	11.5	13.8	13.3	12.5	11.3	
Male schizophrenics ..	16.8	16.0	18.1	16.8	16.8	17.0	16.8	16.4	
Female schizophrenics	17.8	16.8	14.3	16.3	16.1	14.5	15.4	15.3	

Relationships for this variable, which was tentatively considered to be akin to "over-inclusion", are of a very much different and simpler kind. Firstly

it is noted that, although only 10 patterns were shown during recall, group means exceed the score of 10 for all conditions. In other words, neurotics as well as schizophrenics "over-include" relative to the recall stimulus number. Schizophrenics, however, score significantly higher than neurotics (Table IV), which is the only significance found.

4. *Adequacy of certainty.* Adequate attribution of feelings of certainty to correct and wrong recognition responses is shown in Tables VII and VIII.

TABLE VII
Means of Adequacy of Certainty, Pertaining to Table VIII

Test					1	2
Male neurotics	..	..	..	..	33.0	33.5
Female neurotics	..	..	..	..	48.1	54.7
Male schizophrenics	..	..	..	..	-13.8	-4.0
Female schizophrenics	..	..	..	..	23.0	10.8

TABLE VIII
Analysis of Variance for Adequacy of Certainty, Pertaining to Table VII

Source				d.f.	Sums of Squares	Mean Squares	F-ratio
Tests	..	..		1	138.063	—	N.S.
Groups	..	..		1	47,073.348	—	18.260‡
Sex	..	..		1	15,396.674	—	5.972*
T × G	..	..		1	7,178.375	—	N.S.
T × S	..	..		1	95.061	—	N.S.
G × S	..	..		1	470.222	—	N.S.
Residual	..	..		137	353,181.917	2,577.970	—
Total	..	..		143	423,533.660	—	—

Significance: * = 5 per cent.; ‡ = 0.1 per cent.

The expectation is borne out that schizophrenics are less adequate than neurotics in attributing correct confidence to performance.

5. *Reliability.* Before discussing the correlations obtained with certainty measures, their reliabilities are shown in Table IX.

TABLE IX
*Uncorrected Repeat Reliabilities of Test Scores
(24 Hours Interval, 48 Neurotics)*

				30 Seconds Recall	2 Seconds Recall	Recognition
Degree of certainty	..	..		0.59	0.75	0.84
Positive extreme certainty	..	..		0.83	0.80	0.88
Negative extreme certainty	..	..		0.74	0.71	0.48
Subjective recognition	..	..		—	—	0.80
Adequacy of certainty	..	..		—	—	0.34

For degree of certainty reliability increases with the differentiation obtained between neurotics and schizophrenics (Fig. 2). This might suggest that reliability is a function of the degree of validity. This, however, is contradicted by the reliabilities of positive extreme certainty which are equally high for all conditions despite marked variation in differences between neurotics and schizophrenics. Reliabilities for negative extreme certainty are most likely

underestimates, as resulting from too few responses (Fig. 3, medium portion), distributions were irregular or skewed, particularly for the 30 seconds recall and recognition conditions. One reason for the low reliability of adequacy has already been given as due to scoring procedure (see Method section). However, genuine individual variations between tests 1 and 2 may have occurred despite relatively stable means. This is indicated by some considerable changes in validity coefficients noted in Table X. With these reservations in mind, the obtained reliabilities are quite satisfactory and agree well with the high odd-even (38) and split-half (31) reliabilities as found by other authors.

6. *Certainty and questionnaire criteria.* The correlations between test and questionnaire scores are shown in Table X. Distributions for scores 7 and 9 were irregular and skewed, with many subjects having no score. A number of scattergrams revealed curvilinear relationships, discussed in the following section.

TABLE X
Correlations Between Certainty Test Scores and Questionnaires (48 Neurotics)

			Friendship Check List				
			Extra- version	Neuro- ticism	Rigidity	Positive q- Extremes	Negative q- Extremes
1	Degree	30 seconds recall	0.29*	-0.18	0.21	0.03	-0.22
2	of	2 seconds recall	0.32*	-0.01	0.23	0.06	-0.21
3	Certainty	Recognition	-0.09	0.05	0.38†	0.45†	0.15
4	Positive	30 seconds recall	0.27	-0.09	0.23	0.12	-0.18
5	c-	2 seconds recall	0.27	0.04	0.24	0.11	-0.16
6	Extremes	Recognition	-0.10	0.08	0.46†	0.39†	0.24
7	Negative	30 seconds recall	-0.29*	0.14	-0.15	0.09	0.26
8	c-	2 seconds recall	-0.36*	0.10	-0.19	0.05	0.11
9	Extremes	Recognition	-0.18	0.13	0.13	-0.22	0.02
10	Subjective recognition	..	-0.06	0.13	0.12	0.05	0.19
11	Adequacy, test 1	..	-0.04	-0.03	-0.04	-0.37†	-0.37†
12	Adequacy, test 2	..	0.07	-0.05	-0.27	0.02	0.10

Significance: * = 5 per cent.; † = 1 per cent.; c-extremes = certainty extremes; q-extremes = extreme questionnaire responses.

As regards positive certainty and q-extremes, correlations are significant only for positive attitude during recognition. Negative q-extremes, which correlate negatively with certainty during recall, turn also positive during recognition. This reversal, which is significant at the 5 per cent. level, confirms the outstanding importance of the situation employed during recognition for the measurement of set. This point is further emphasized by the similarity of results between rigidity and positive q-extremes, both of which have fulfilled the expectation to correlate significantly with certainty, and that under the same conditions. The task remains now to identify the attributes of recognition which favour the demonstrated increase in generality of set. A second, not less important task, is to find a common denominator for the variables which assume similar types of reaction under such conditions.

Extraversion and neuroticism, taken separately, are not likely to fall under this denominator. Extraversion, which correlates significantly and in the expected direction with certainty during recall, plays no important part during recognition. During recall the signs of coefficients are consistent between

rigidity and extraversion, during recognition they are opposite. As regards neuroticism, all correlations remained low and inconsistent.

7. *Curvilinear relationships.* As was stated in the introduction, curvilinear relationships are not infrequent in motivational research. To investigate this aspect, the scattergrams between certainty and questionnaire scores were inspected. A number of curvilinear relationships were found and tested for significance using the following method. Firstly, male and female groups were divided into subgroups of low, medium and high extraversion, neuroticism, etc. Low and high subgroups were subsequently combined and compared with the medium subgroup as regards the number of subjects falling above or below the fiftieth percentile of the scores investigated. Significance was tested by means of χ^2_c , the square root of chi-square, with Yates' correction for continuity applied (14, 15). Because of existing sex differences this operation was performed separately for male and female subgroups. No consistent or significant curvilinear relationships were obtained for extraversion, neuroticism and negative q-extremes.

For rigidity and positive q-extremes, a consistent and similar set of results emerged, as seen in Table XI.

TABLE XI

4 Number of Relationships Between Rigidity and Positive q-Extremes, on the One Hand, and Measures of Test Certainty, on the Other, Tend Toward Curvilinearity

Score Relationships and Chi _c Values			Rigidity	Positive q-Extremes (Friendship Check List)
1	Degree	30 seconds recall	Linear	Curvilinear, 1·53
2	of	2 seconds recall	Curvilinear, 2·14*	Curvilinear, 2·76†
3	Certainty	Recognition ..	Linear	Linear
4	Positive	30 seconds recall	Curvilinear, 1·53	Curvilinear, 1·53
5	c-Extremes	2 seconds recall	Curvilinear, 2·14*	Curvilinear, 2·14*
6		Recognition ..	Linear	Linear
7	Negative	30 seconds recall	unscorable	unscorable
8	c-Extremes	2 seconds recall	Curvilinear, 1·53	Curvilinear, 2·14*
9		Recognition ..	Linear	Linear
10	Subjective recognition ..	..	Curvilinear, 1·53	Linear
11	Adequacy, test 1 ..	..	Curvilinear, 2·14*	Curvilinear, 1·53
12	Adequacy, test 2 ..	..	Linear	Linear

*=2·5 per cent.; †=0·5 per cent. level of significance.

For a χ^2_c of 2·76: $m=3$, $p=0\cdot25$.

For a χ^2_c of 2·14: $m=4$, $p=0\cdot33$.

For a χ^2_c of 1·53: $m=5$, $p=0\cdot42$.

All shorter tail tests.

In the case of recognition all relationships are linear. During recall there is a slight tendency towards curvilinearity for the 30 seconds exposure condition. For the 2 seconds exposure, nearly all relationships are significantly curvilinear. The direction of these relationships is as follows. For degree of (positive) certainty and positive c-extremes the medium subgroups were consistently more confident than the low and high subgroups. For negative c-extremes the subgroups scored higher or were less confident than the medium subgroup.

8. *Correspondence between rigidity in neurotics and the neurotic/schizophrenic dichotomy.* In the introduction it was proposed to test the hypothesis of a dimension of certainty, whereby certainty increased with rigidity in neurotics and reached its highest score in the schizophrenic group. As our main

interest is in the interaction between the experimental conditions and individual differences, the discrimination between the neurotics and schizophrenics is presented first in Table XII, separately for all conditions used.

TABLE XII

Critical Ratios of Differences Between Neurotics and Schizophrenics, Pertaining to Figure 2 (upper part) and Figure 3 (lower part). Significance Increases from 30 Seconds Recall through 2 Seconds Recall to Recognition. (N=24 Neurotics and 12 Schizophrenics in Each Subgroup, Separated for Sex.)

Condition		Recall				Recognition			
		30 Seconds		2 Seconds		30 Seconds		2 Seconds	
Exposure		1	2	1	2	1	2	1	2
Test		1	2	1	2	1	2	1	2
Degree of	m	0.34	1.22	2.51*	2.77†	3.68‡	3.10†	3.75‡	4.00‡
Certainty	f	1.53	1.65	4.18‡	4.80‡	3.08†	4.12‡	5.20‡	4.09‡
Positive									
Extreme	m	0.10	1.35	1.22	2.02*	3.84‡	3.59‡	3.62‡	3.87‡
Certainty	f	1.15	0.87	2.00*	2.92†	3.22‡	4.54‡	5.19‡	4.77‡

m=male; f=female. d.f.=570 throughout. Significance: * = 5 per cent.; † = 1 per cent.; ‡ = 0.1 per cent.

The clear result emerges from this table that the significance of differences increases steadily and enormously from 30 seconds recall through 2 seconds recall to the recognition conditions, which are fairly similar among themselves. When comparing these results both with the correlations between rigidity and certainty, shown in Table X, and with the score relationships described in Table XI, two problems emerge.

During recall, differences between neurotics and schizophrenics increase significantly from 30 seconds to 2 seconds exposure (Table XII). In the neurotic group there is no corresponding difference in the size of coefficients shown in Table X. Moreover, the curvilinear relationships for rigidity (Table XI) indicate that the medium instead of the high rigid subgroup is most confident. For recall the hypothesis tested must therefore be discarded.

For recognition, the situation is different and definitely in favour of the hypothesis. Here the difference between neurotics and schizophrenics is highest (Table XII), the correlation between rigidity and certainty is positive and greatest (Table X), and the respective score relationships are linear (Table XI). For the sake of brevity, it is simply stated that the means of all certainty recognition scores increased in the order: low rigid—medium rigid—high rigid neurotics—schizophrenics, for males and females alike.

9. *Intercorrelations of test scores.* A complete matrix of intercorrelations of test scores is shown in Table XIII.

Intercorrelations between conditions, but *within* the three groups of certainty scores 1-3, 4-6, and 7-9 differ considerably between recall and recognition. The mean coefficient between 30 and 2 seconds recall (1 vs. 2, 4 vs. 5 and 7 vs. 8) is 0.63, and between recognition and the two recall conditions 0.29. A similar observation is made for the correlations *between* the above three groups of certainty scores. All possible intercorrelations between 30 and 2 seconds recall average a coefficient of 0.60, the mean coefficient for the combination 2 seconds recall/recognition is 0.32 and, for 30 seconds recall/recognition 0.27. When tested for significance these differences are generally significant, either at the 5 or 1 per cent. level. As they can hardly be attributed to corresponding differences in reliability (Table IX), the suggestion is made that

TABLE XIII
Intercorrelations of Test Scores

	2	3	4	5	6	7	8	9	10	11	12
1 Degree of Certainty	0.66†	0.20	0.88†	0.60†	0.24	-0.87†	-0.60†	-0.28*	-0.14	-0.03	-0.11
2 30 seconds recall	—	0.30*	0.63†	0.87†	0.27	-0.57†	-0.88†	-0.26	-0.04	-0.06	-0.14
3 Recognition		—	0.40†	0.47†	0.84†	-0.08	-0.15	-0.52†	0.05	-0.32*	-0.36*
4 Positive c-Extremes			—	0.70†	0.32*	-0.64†	-0.52†	-0.33*	0	-0.13	-0.21
5 30 seconds recall				—	0.47†	-0.45†	-0.61†	-0.29*	0	-0.12	-0.31*
6 Recognition					—	-0.08	-0.11	-0.24	0.07	-0.16	-0.37†
7 Negative c-Extremes						—	0.58†	0.21	0.11	-0.15	0.02
8 30 seconds recall							—	0.22	-0.05	0.03	0.06
9 Recognition								—	0.16	0.10	0.29*
10 Subjective recognition									—	-0.25	-0.19
11 Adequacy, test 1										—	0.34*
12 Adequacy, test 2											—

N=48 neurotics. Significance: * = 5 per cent., † = 1 per cent. level.

the character of certainty during recognition is considerably different from that found during recall, a point which has already been repeatedly made.

For subjective recognition none of the correlations obtained are significant. The relationships between adequacy and positive certainty are consistently negative, whereas those to negative certainty are, with one exception, positive. Significantly enough, these correlations are highest for recognition. While the judgment of highly confident subjects may be expected to be relatively inadequate, the correlations quoted provide only a first rough guide, as non-linear relationships tended to emerge. To analyse this problem more fully, much larger samples are required.

DISCUSSION

Judging by the insignificant between-test variations (Tables IV, upper part, and V) and high repeat reliabilities (Table IX), certainty appears a highly stable behaviour for any one of the conditions employed. In strong contrast to this, considerable variations between conditions are found for means, group discrimination, validity coefficients and intercorrelations. Differential effects important to the investigation of personality are those connected with exposure time in recall and the difference between recall and recognition conditions. The slight differences found between the two recognition conditions may be due to lack of differential stimulatory effects between the 30 seconds and 2 seconds presentations. As recall took place after each individual presentation of a pattern and recognition after an entire set of 10 patterns had been presented, the difference in exposure time may have remained ineffective only for recognition.

Further differential effects between recall and recognition are: curvilinear relations for recall as against linear relations for recognition (Table XI); considerable difference in validity coefficients (Table X), some of these being of a qualitative kind (extraversion and negative q-extremes), and significant differences in intercorrelations of test scores (Table XIII).

The general conclusion pertaining to the main aim of the present investigation is drawn that the personality validity of certainty changes fundamentally as a result of the interaction between definable stimulus conditions and individual differences disposed to act in a certain manner. A more detailed operational definition of some of the stimulus, personality and also of situational disposition variables appears a pre-requisite for the elimination of alternative hypotheses and, consequently, for further theoretical developments. Amongst the stimulus variables, exposure time, level of difficulty and stimulus ambiguity are of obvious importance. As regards situational disposition variables, attention will concentrate on less tangible factors like response ambiguity, ego-involvement and "felt difficulty" in doing the test.

The second main aim, that of finding independent personality criteria of certainty, has been met with encouraging results. Apart from the confirmation of the predictions made for the neurotic/schizophrenic dichotomy, some of the questionnaires scores have yielded new results. Rigidity and positive q-extremes correlated highest with positive certainty during recognition (Table X), and they are the only criteria to have produced U-shaped relationships with certainty scores. In view of the fact that such relationships are usually found to be associated with "ego-involving" conditions (22), or motivational states bearing other labels (21, 27), these results add to the hypothesis that the present scores of rigidity and response set measure some common factor of drive intensity.

This point touches on the problem of the relative importance of the various personality criteria employed, which was the third main aim of the present experiment. In this respect the review on motivation of Inglis (21), who based his interpretations on Eysenck's factors of extraversion and neuroticism, may be considered. As does the present author, Inglis accepts confidence judgment of performance as an indicator of "motivational intensity" and/or "drive strength". The correlations between extraversion and certainty (Table X) are significantly in the expected direction in the case of recall. However, the negative correlations for recognition (Table X) and the absence of curvilinear relationships will certainly indicate extraversion to be of restricted and qualitatively different importance. As regards neuroticism, the present results do not support the idea that this criterion may be taken as a measure of drive-strength of the kind investigated. However, a final judgment cannot be made until the *composite* effect of these two criteria is analysed. To do this, much larger samples are needed.

The significant problem of sex differences is only pointed to. The present results confirm largely what is already known or expected to occur. Sexes differed significantly as regards certainty (Table IV), extreme certainty (Table V) and adequacy of certainty (Table VIII). The overall consistent picture was that males scored higher on certainty, positive c- and q-extremes and on rigidity, but lower on adequacy of certainty.

If the hypothesis is accepted that rigidity and positive q-extremes, as used in the present experiment, have motivational character the curvilinear relationships found for recall may be interpreted in analogy to Iverson and Reuder (22), as follows. Low motivation has an inhibitive and high motivation a disruptive effect on feelings of confidence, while medium levels facilitate confidence. As curvilinearity and task difficulty increased from the 30 seconds to the 2 seconds recall condition, these two characteristics may be said to be functionally related, whereby the optimum point of the confidence/motivation relationship may shift with the degree of difficulty.

This role of task difficulty in the formation of certainty functions is likely to be of a mediating rather than constitutive kind. To make this clear the plausible assumption is first made that certainty, in general, decreases with difficulty, as it does from 30 seconds to 2 seconds recall (Fig. 2, upper part). It may then be seen that although recognition of the present kind is extremely difficult—the schizophrenic mean error was, for example, greater than expected by chance—the corresponding certainty score of the schizophrenics is highest of all conditions, including the easy 30 seconds recall test. With regard to curvilinearity, it is observed that recognition certainty in the neurotics falls between the 30 seconds and 2 seconds recall levels. If this indicated a medium level of task difficulty curvilinear relationships should have been found for recognition, as they were for the lower and higher recall certainty levels. Both these findings contradict the hypothesis that difficulty level is a primary constituent of the variations in certainty found.

The differences in score regression found between recall and recognition, finally, permit the following speculations. During recall there is a high degree of subjective awareness as to which shapes are correctly placed in the drawings and which not (relative absence of ambiguity). As spontaneous reactions of the subjects show "felt difficulty" and ego-involvement to be highest for the more difficult 2 seconds recall test, the curvilinear relationships found (Table XI) agree well with independent reports (22). During recognition it is extremely difficult to judge where one is right or wrong. Though responses are made

without great difficulty they are frequently qualified as "pure guesses". This, in contrast to recall, indicates a high degree of ambiguity, which has already been stated to facilitate the appearance of extreme response set (8) and therefore possibly also of linearity of the types of regression discussed. This ambiguity hypothesis would explain the high degree of confidence for recognition which was obtained despite the high degree of task difficulty, as discussed above. It would, furthermore, provide a direct link between extreme response set and the theory of motivation.

SUMMARY

Measures of degree, extremeness and adequacy of certainty in recall and recognition were obtained from neurotics and schizophrenics. The memory tests concerned were presented under conditions of short and long exposure time. With fairly high repeat-reliabilities and non-significant between test variations, certainty varied considerably and systematically with the conditions employed. Similarly, differentiation between the two groups of patients was dependent upon the same conditions.

Personality questionnaires, completed by the neurotics, provided the following main results. No consistent or significant results were obtained with a scale of neuroticism. Extraversion correlated significantly with certainty only under conditions described as low in ambiguity. The most significant results were obtained with two scales intended to assess "motivational intensity", one of which measuring extreme response set. The regression of certainty on these variables was curvilinear under conditions of low ambiguity and linear for ambiguous conditions. Results were analysed mainly from three points of view: to find conditions suitable for the measurement of certainty, to create independent criteria of certainty and to assess the relative interaction of various personality questionnaire scores with certainty.

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APPENDIX

Nigniewitzky Rigidity Scale:

1. You make it a matter of principle never to permit your friends to come between you and your work.
2. It is impossible to expect people of different natures to become friends.
3. Before undertaking a trip, you always plan well in advance and decide on an exact itinerary, from which you are reluctant to deviate.
4. You tend to become so completely absorbed in the work you are doing that you don't like to be interrupted and have to change to something new.
5. You have arrived at a conclusive philosophy of life by which you view the scheme of things in their true light.
6. The realization of a man's creative work is of greater value than leisure and enjoyment of everyday human relations which are not important.
7. You always prefer the familiar, the safe and sure, to taking chances with the novel and untried.
8. A sound person will always judge another individual's behaviour according to what he would do himself.
9. Whenever the occasion arises to broaden your experience (new job, new residence, travel, etc.), your natural disposition "to belong" makes you reluctant to undertake a change.
10. You are not the kind of person who has a blind faith in human nature; you find it is better, rather, to be realistic and question people's intentions to be sure of them before becoming involved in friendship.
11. The following essentials: will-power, determination, perseverance, and independence are necessary and sufficient to make a true success of one's life.
12. You never feel that you are able to really enjoy yourself with friends until you have done your duty.
13. A man ought to concentrate on one task at a time for the sake of doing a thorough job, otherwise he should not undertake it at all.
14. You are most reluctant to start something new with which you are unfamiliar.
15. Friendship goes on the rocks when one of your friends neglects you and takes you for granted.
16. There are only a few styles and colours of clothes that you have found to your satisfaction and you would be most reluctant to change to something new.
17. You find it extremely bothersome when unexpected visitors invade your privacy without previous warning.

18. Although you are reluctant to admit it, speaking very frankly, you sometimes think that you can handle almost anything in your field of work.
19. You never compromise on quality but only buy the most solid and lasting things or you do without.
20. Some of your most worthwhile experiences have been (or would be) in associations and talks with persons of great prestige in your field of work.
21. People who seem unsure and uncertain about life make you feel uncomfortable.
22. It is when a man makes of his life a personal ideal and never deviates from his own principles that he is set apart as a unique and worthwhile individual.
23. It is most disconcerting to find that your favourite barber, who always serves you, is not available.
24. You can only be on close and personal terms with a person when you are sure of his genuine devotion and have grounds for trusting him.
25. A person who does not live up to the standards he sets for himself does not deserve our sympathy.
26. You believe in keeping associates at a proper distance because familiarity breeds contempt.
27. After writing and sending an important letter, you often go over in your mind the possible replies and make alternative plans for action accordingly.
28. Generally you are indifferent to people but you are devoted to a few close to you and dislike intensely certain others.

All items are of the "true-false" type and are scored in the positive direction.

NARCOLEPSY AND HYPOGLYCAEMIA*

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FOR some years now it has been recognized that the symptoms induced by hypoglycaemia may resemble narcolepsy. Cases of islet-celled pancreatic adenomas simulating narcolepsy have been described by Harris (7), Delay (4) and Wyke (18). The resemblance may be a superficial one, however, and Wyke observed in his case during a "sleepy attack" that "the EEG pattern bore no resemblance to that of natural or artificial sleep". In a more recent paper Ziegler and Presthus (19) described thirteen patients who showed normal EEGs at blood glucose levels (induced by fasting and intravenous insulin) of from under 15 mg. per cent. to 54 mg. per cent. In eight of these patients there were no clinical symptoms; in the remaining five patients the symptoms were described as follows: "feel like a ton of lead", "warm and sleepy", "tired", "drowsy and sweaty" and "dizzy and tired". It seems, in fact, that the clinical description of drowsiness in hypoglycaemia may correspond either to no EEG changes or to various degrees of slowing rather than to the typical recurrent light sleep patterns characteristic of narcolepsy. It will be recalled that hypnosis too is ushered in by a sense of drowsiness but that the EEG changes are not those of sleep (1, 2, 5). Many hysterical trance states—often loosely, and unfortunately, described as narcolepsy—come into this category. In short, it is apparent that all that sleeps is not narcolepsy (6, 12, 13, 14).

In this paper we should like to describe the EEG changes occurring under hypoglycaemia in five cases of true narcolepsy. In a previous paper devoted mainly to the clinical aspects Smith (13) drew attention to an apparent alerting effect in the EEGs of two narcoleptics who were rendered hypoglycaemic by intravenous insulin (Cases 1 and 2 below). Since then three cases have been added and we would like to describe and discuss the phenomenon in more detail.

METHOD

The method, which was developed by Hoffer (9) as part of a homeostatic battery of tests in psychiatric patients, consists in injecting intravenously one unit of soluble insulin per eight pounds body weight in the fasting subject. Prior to the injection the EEG is taken for twenty minutes; afterwards blood

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is taken (for glucose) four times at ten minute intervals and the EEG changes are recorded. About fifty minutes after the insulin injection hypoglycaemia is terminated by the I.V. injection of 50 c.c. 33 $\frac{1}{3}$ per cent. glucose and further EEG recordings are taken for twenty minutes following which the patient is returned to the ward.

The EEG examinations were carried out on a Grass Model 3D console eight-channel apparatus. Silver disc electrodes applied with collodion were used. Parallel E.K.G. examinations were recorded simultaneously on lead II or lead III. No sedatives or stimulants were given for the preceding twenty-four hours.

RESULTS

The EEG and clinical changes under hypoglycaemia were as follows:

Case 1. Female, aged 27, with narcolepsy, cataplexy, sleep hallucinosis and paranoid psychosis.

The pre-insulin EEG showed no abnormality. It was difficult to keep the patient awake during hyperventilation but when this was done no abnormality appeared. Seventeen units of soluble insulin was given at 8.55 a.m. Blood glucose findings were as follows: 8.45 a.m. (fasting) 108 mg. per cent., 9 a.m. 88 mg. per cent., 9.10 a.m. 50 mg. per cent., 9.20 a.m. 38 mg. per cent., 9.30 a.m. 64 mg. per cent. I.V. glucose was given at 9.37 a.m.

From 8.55 a.m. on it was noted that she was not nearly so drowsy. Some drowsiness was noted at 9.04 a.m. and 9.08 a.m., but at 9.10 the recording was essentially normal except for some frontal slowing at 5-7 per second. At 9.17 she was wide awake and had a well-maintained alpha rhythm. At 9.28 she was still awake but the amplitude of the alpha rhythm and other waves was dropping slightly. Some drowsiness appeared with hyperventilation. At 9.37 a.m. glucose was given I.V. and at 9.40 a.m. her previous intermittent, excessive drowsiness had reappeared.

Clinically she felt less drowsy at the lower blood-glucose levels.

Comments. This patient showed a definite diminution of drowsiness under hypoglycaemia and an abrupt return of drowsiness following glucose. The diminution in drowsiness appeared to run roughly parallel to the blood-glucose levels.

Case 2. Male, aged 36 years, with narcolepsy and cataplexy.

The pre-insulin EEG showed no abnormality apart from some drowsiness. As the blood glucose dropped from 93 mg. per cent. to 39 mg. per cent. little or no drowsiness was observed. At 9.30 a.m. the blood glucose was 39 mg. per cent. At this time generalized theta activity along with some generalized delta activity appeared from all leads. At 9.48 a.m. I.V. glucose was given and at 9.50 a.m. drowsy patterns again appeared along with normal brain activity. Drowsiness at this time was so marked that the drowsy patterns appeared almost immediately upon closing of the eyes.

Clinically he was less drowsy at the lower blood-glucose levels.

Comments. This patient also, like Case 1, showed a diminution of drowsiness under hypoglycaemia and an abrupt return of drowsiness following glucose. It has since been discovered that this patient was inadvertently given methamphetamine hydrochloride 5 mg. at 7 a.m. Despite this the changes are striking.

Case 3. Male, aged 22 years, with narcolepsy and vivid, disturbing dreams.

The pre-insulin EEG showed a stable recording with considerable excessive, intermittent drowsiness and slight frontal slowing. After injection of 23 units soluble insulin at 8.48 a.m. the blood glucose fell to its lowest level of 30 mg. per cent. at 9.10 a.m. At this time frontal slowing was still present but had not increased. Drowsy activity had decreased considerably. At 9.13 a.m. generalized slowing from all leads was noted in the form of 6-7 per second activity. At 9.20 a.m. blood glucose was 36 mg. per cent. and the patient complained of some sleepiness and palpitation. At 9.30 a.m. blood glucose was 72 mg. per cent. and the generalized slowing had decreased. Glucose was then given intravenously following which drowsiness was noted to appear much more readily.

Clinically the changes were not striking until glucose was given when he felt much more sleepy.

Comments. This case was less impressive than Cases 1 or 2 but nevertheless there was considerable reduction in drowsiness on the EEG at the lower blood-glucose levels and drowsiness was noted to be increased after intravenous glucose was given.

Case 4. Male, aged 40 years, with narcolepsy, cataplexy, and vivid, disturbing dreams.

During the pre-insulin part of the recording the patient had great difficulty in staying awake. The fasting blood glucose was 99 mg. per cent. and at 8.50 a.m. 25 units of soluble insulin was given intravenously. No change appeared in the EEG until the blood glucose dropped to 43 mg. per cent. At this time generalized slowing maximal in the frontal regions appeared. A considerable amount of drowsy activity was present then but as the blood glucose dropped to 27 mg. per cent. very little drowsy activity was noted. At 9.42 a.m. I.V. glucose was given and the recording at 9.45 a.m. again showed excessive drowsiness.

Clinically the patient reported feeling less drowsy at the lower blood-glucose levels.

Comments. This recording follows the general pattern of the others although the diminution in drowsiness under hypoglycaemia is less striking than Cases 1 and 2.

Case 5. Male, aged 31 years, with narcolepsy, cataplexy, sleep paralysis and sleep hallucinosis.

As in the previous cases the only pre-insulin EEG change shown was excessive drowsiness reappearing within seconds after the patient had been roused. The fasting blood glucose was 94 mg. per cent. At 8.50 a.m. 26 units soluble insulin was injected intravenously. At 9.03 a.m. the recording showed almost no drowsy activity; the blood glucose was then 70 mg. per cent. At 9.10 a.m. the blood glucose was 49 mg. per cent. and the recording showed no drowsiness. At this time, however, background activity showed some instability and occasionally theta activity at 7 per second in short bursts was observed. A few minutes later more generalized slowing with fairly consistent theta activity was noted. At 9.36 blood glucose had risen to 58 mg. per cent. and the background activity was becoming more stable. There was still no drowsy activity. At 9.46 a.m. glucose was given I.V. and by 9.50 a.m. drowsiness was again being observed. This increased throughout the subsequent recording.

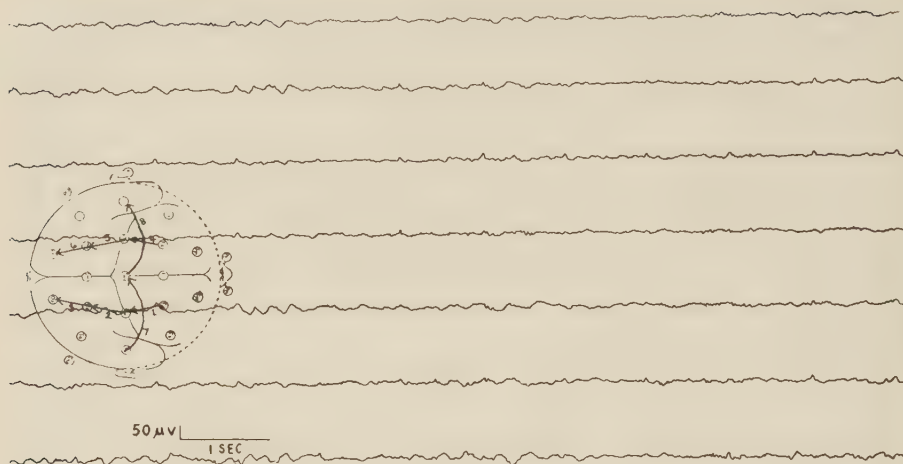


FIG. 1.—EEG of Case 5 prior to insulin injection. Leads as shown. The blood glucose level is 94 mg. per cent. There is much drowsiness.

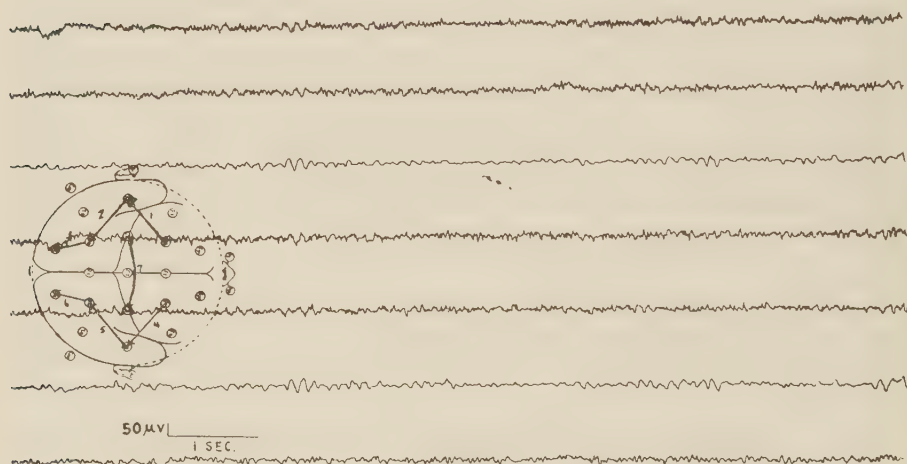


FIG. 2.—EEG of Case 5 twenty minutes after 26 units soluble insulin was injected intravenously. Time 9.10 a.m. Blood glucose 49 mg. per cent. There is no drowsiness. E.K.G. lead III is also shown.

Clinically the patient felt less sleepy after the insulin injection and became more so following the glucose injection.

Comments. This man showed little or no drowsiness on the EEG after the insulin was given until hypoglycaemia was terminated by glucose when his previous drowsiness rapidly returned. Samples of the recording are shown in Figures 1-6.

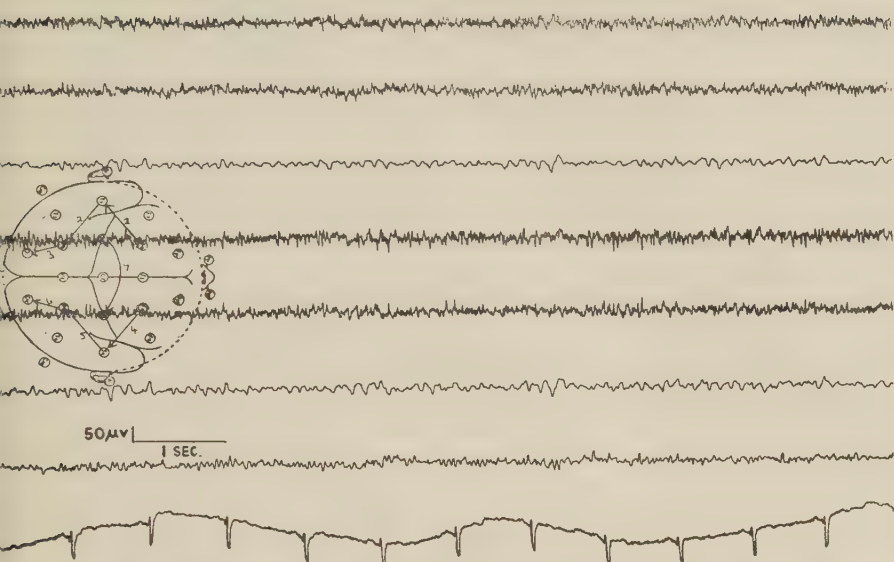


FIG. 3.—EEG of Case 5 at 9.23 a.m. Blood glucose is now 40 mg. per cent. There is no drowsiness.

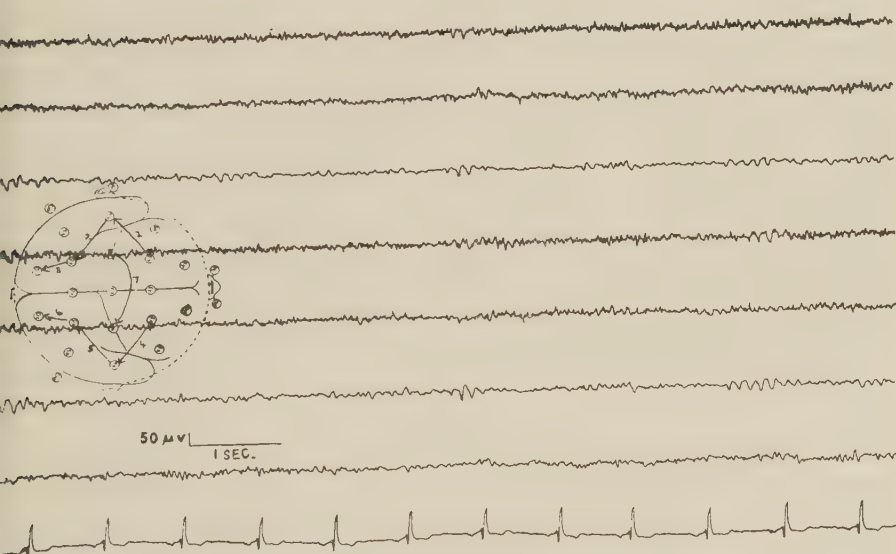


FIG. 4.—EEG of Case 5 at 9.36 a.m. Blood glucose 58 mg. per cent. There is still no drowsiness.

DISCUSSION

The first question which arises in connection with these findings is: are they non-specific, e.g. were the factors of interest and boredom on the frequent taking of blood samples alone responsible for the changes? Against this the following considerations may be urged.

1. An EEG sleep study was carried out on each case after the technique described by Hoffer (8) and Szatmari and Schneider (15). This involved three

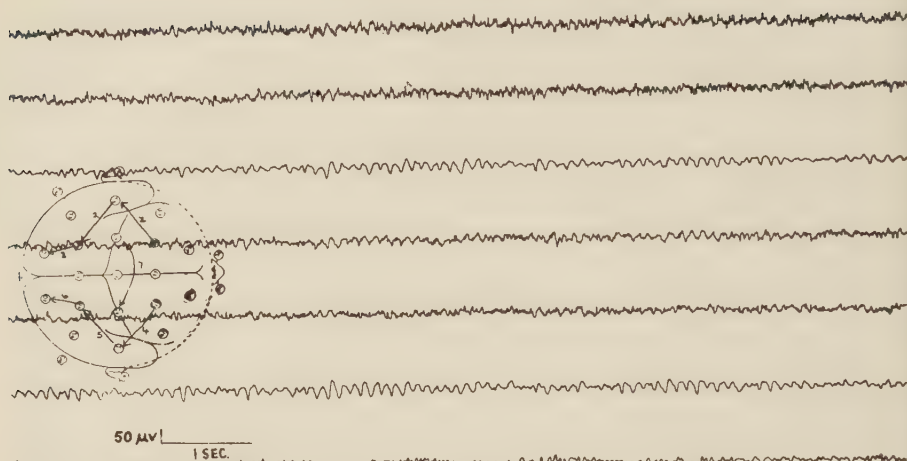


FIG. 5.—EEG of Case 5 one minute after I.V. glucose has been given. Drowsiness has not returned.

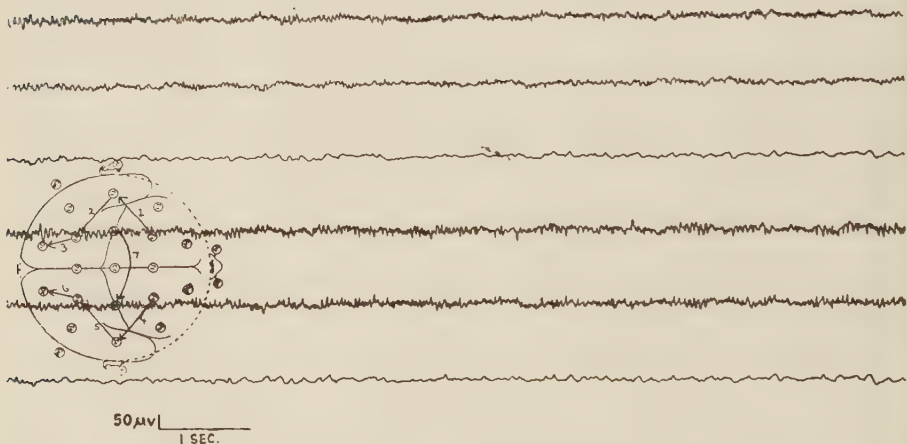


FIG. 6.—EEG of Case 5 six minutes after glucose injection. Some drowsiness is again being observed.

injections: 3 mg. atropine I.M. followed in thirty minutes by 2 mg. physostigmine* I.M. and in a further fifteen minutes by 200 mg. acetylcholine I.V. These injections, though disagreeable, produced very little alteration in the amount of drowsiness present.

* In the published reports of these workers prostigmine was used; later physostigmine was substituted as it had fewer side effects.

2. The drowsy patterns were removed only very temporarily by other interruptions, e.g. talking, etc., in fact for little longer than the actual stimulus lasted.

3. The diminished drowsiness was most pronounced at lower levels of hypoglycaemia.

4. Sleepiness reappeared very rapidly when I.V. glucose was given.

5. Clinically four of the patients noted some diminution of drowsiness under hypoglycaemia; this is the opposite effect to that which might ordinarily be expected.

6. Suggestion was carefully avoided; in fact the results in the first two cases took us entirely by surprise.

There is, then, some reason to believe that the effect was a specific one. It is possible that it may have been due to central liberation of adrenaline with stimulation of the reticular activating system. This explanation would certainly fit in well with the facts that:

(a) Hypoglycaemia is well known to produce increased output of adrenaline.

(b) The reticular activating system is adrenaline-sensitive (10, 11).

(c) The sympathomimetic amines are effective in narcolepsy.

(d) There is a good deal of evidence suggesting a dysfunction of the reticular activating system in narcolepsy (3, 13, 14).

It is interesting to recall in this connection that Weitzner (16, 17) used insulin hypoglycaemia as a treatment in one case of narcolepsy and one of sleep paralysis and claimed to have obtained encouraging results. Weitzner believed that the beneficial change may have been due to an action on the sleep regulating centres in the hypothalamus.

If the findings prove to be peculiar to narcolepsy then they will be of considerable interest since, apart from the EEG changes (excessive drowsiness), there has been a remarkable paucity of constant specific findings in narcolepsy (13). In these five patients the only other laboratory findings of note were some mild disturbances of the Exton-Rose glucose tolerance test in Cases 1-4 (see Table I). Clinically, four of the patients were overweight but otherwise there was no abnormality.

TABLE I

Results of Exton-Rose Glucose Tolerance Test on Five Narcoleptics
(Folin-Wu Method; Normal Fasting Glucose 80-120 mg. per cent.)

Blood Glucose (mg. per cent.)	Case 1	Case 2	Case 3	Case 4	Case 5
Fasting level followed by 50 g. glucose orally	156	97	90	104	103
1 hour level followed by 2nd 50 g. glucose orally	133	134	119	128	159
1 hour	82	172	160	183	143
3 hours	116	113	126	87	79
5 hours	113	62	84	98	94

SUMMARY

The relationship between hypoglycaemia and narcolepsy is briefly discussed. It is pointed out that subjective complaints of "drowsiness" in hypoglycaemic patients are, in many cases at least, not accompanied by drowsy patterns on the EEG.

The EEG changes under hypoglycaemia of five typical narcoleptic patients are then described. All five patients showed diminution of drowsiness in the EEG at low blood-glucose levels and a prompt return of drowsiness following administration of 50 c.c. of 33½ per cent.

glucose intravenously. It is argued that this effect was probably a specific one and that it may have been due to liberation of adrenaline with stimulation of the reticular activating system.

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OBSERVATIONS ON THE PSYCHOTOGENIC EFFECT OF N-N DIETHYLTRYPTAMINE, A NEW TRYPTAMINE DERIVATIVE

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SINCE Evarts, Fabing and Marrazzi reported on bufotenine and Szára on N-N dimethyltryptamine (DMT) the tryptamine derivatives have been arousing increased interest. A team of the above Institute was the first to test DMT and N-N diethyltryptamine (DET) in man. These compounds were synthesized by Szára, by the method of Speeter and Anthony. DET was then synthesized again and analysed pharmacologically by Borsi and Lénárt.

Our aim was to elucidate the eventual toxic or psychotropic effects of the single metabolites of tryptophane in order to gain possible insight into the genesis of the metabolic disturbance in schizophrenia. In contrast with the so-called plantagenic psychomimetics (LSD, mescaline, etc.), the tryptamine derivatives seem to belong to the "somatogenic" group, which may be formed under special conditions also in the animal organism (de Boor). Thus far, bufotenine is the best known one among these compounds. This agent is hallucinogenic, occurs widely in nature and, according to Fabing, may be formed also in man as a result of abnormal serotonin breakdown. The intestinal bacteria may give rise to tryptamine formation from tryptophane by pathological decarboxylation. Tryptamine, according to Baruk and Nieuwhuysen, may act as a psychomimetic, although it usually elicits only psychomotor disturbances in animals. In the presence of a transmethylase enzyme system DMT may be formed from tryptamine even in the body. Although all these are merely speculations, the fact that similar metabolites are actually formed from tryptophane and these are closely related in structure to serotonin indicates that the experiments with these agents seem to have much greater significance in our effort to elucidate the biochemical background of endogenous or exogenous psychoses than the experiments with mescaline or LSD. Also according to Marrazzi serotonin or bufotenine, its dimethyl derivative "comes closer than does LSD to the type of endogenous psychotogen that might be a natural cause of some mental disturbances".

The response to DMT, which differs from bufotenine in the absence of just one OH group, has been studied in 30 normal subjects and 24 mental patients. The experimental psychosis caused by 0.7 to 1.0 mg./Kg. of DMT injected intramuscularly begins within 3 to 5 minutes and lasts about 50 to 60 minutes. The characteristic symptoms of this model psychosis are intense vegetative changes, more or less marked anxiety, in some cases

neurological signs indicative of a functional disturbance of the non-dominant hemisphere, disturbances of mood and perception, first of all visual illusions and visions, alterations in the perception of time and space, shorter episodes of tenebrosity and loosened associations. In many of its features this disturbance was similar to the single phases of the LSD effect, but bore no definite resemblance to either the schizophrenic or the other endogenous-psychotic clinical states. It is less suitable than the other psychotogens for the purpose of auto-analysis. In schizophrenics the DMT psychosis is by far less colourful. (For further data the reader should consult the reports by Arnold and Hoffmann, Sai-Halász *et al.*, Böszörményi and Szára.)

The pharmacological data for DET, the other tryptamine derivative, may be outlined as follows. In the mouse the intravenous LD₅₀ is 32 mg./Kg., in the dog it is 24.5 mg./Kg.

The effect on behaviour has been studied in dogs and rats. In 2 dogs 1 mg./Kg. of DET-HCl, injected intramuscularly, produced no noticeable changes. In 1 dog excitation developed about 15 minutes after the injection of 1.5 mg./Kg. intramuscularly. The animal, which had behaved absolutely normally, began to run up and down, did not recognize his master, did not listen to his name and refused food. This lasted about 15 to 20 minutes. A similar response was seen in another dog, which had been given 2 mg./Kg. of DET-HCl. In rats doses of 1 mg./Kg., 2 mg./Kg., 5 mg./Kg. and 7.5 mg./Kg., as administered through the intravenous route, caused excitation depending in grade according to the size of the dose, but no convulsions. The animals ran about, smaller doses caused them to climb, stand on two legs and retreat backwards. The 10 mg./Kg. dose produced clonic convulsions. All the animals survived, the response was 15 to 20 minutes in duration, followed by a depressive phase (Borsi).

In analogy to the LSD compounds, we expected the diethyl derivative of tryptamine (DET) to be more toxic than DMT in man and yet the only difference was that confusion was longer in duration and qualitatively different. These experiments involved 30 normal test subjects and 41 mental cases. The symptomatology of the DET model psychosis, as reconstructed from the protocols and from the subjective accounts given later, may be summarized as follows.

The model psychosis developing in response to 0.70 to 0.80 mg./Kg. of DET intramuscularly may be likened to the mescaline effect of mild or moderate severity, although in some features it resembles LSD intoxication. After the initial unpleasant vegetative symptoms subside, the majority of the test subjects develop a meditative, pleasant, euphoric mood, with the contact maintained. Consciousness is loosened, concentrative power is weak, the perception of space, time and body is disturbed. Other phenomena of depersonalization, as well as visions, illusions and not infrequently synaesthesiae also occur. Increased motor activity and anxiety occurred in only a few (5) normal test subjects.

As an illustrative example, we present the following protocol.

A.B., aged 26 years, a novelist, 0.65 mg./Kg. of DET. Pulse rate 104/min. Blood pressure 190/110 mm. Hg.

10.55. Injection is given.

11.05. Pulse rate 120/min. BP: 200/130 mm. Hg.

11.09. Mild vertigo is noted.

11.16. Pulse rate 100/min. BP: 200/130 mm. Hg.

11.23. He says laughing: "Now comes the attraction, I have the honour of seeing the elements of the universe in this moment. As if I saw algae, flagellates under the

microscope, in white and black. Though it's terrible that I can't express myself properly." 11.26. "Now I see some colours, too. As if I saw a shell, the rainbow colours are disintegrating rapidly. It is the trouble that meanwhile I am too much pre-occupied with myself, I enjoy it greatly, though I cannot concentrate on it well. If I close my eyes I see everything lighter."

11.30. "One's consciousness becomes air-like. From the neck upward I am feeling a shapeless lightness." (3 TAT pictures are shown to him, which he describes banally and shortly.)

11.40. "I am filled with autotelic heaps of words, with autotelic word-magic, I cannot express myself." His face is bright, reflecting euphoria.

"It is a bodyless, stupid, tickling sense of well-being. With my eyes shut I see fungus-shaped phenomena, as if they were clouds after an atomic explosion, in black, white and purple colour."

11.45. Pulse rate 88/min., BP: 200/120 mm. Hg. (He is listening to music by Chopin): "Music does not fascinate me, does not interest me, everything is so very much, one associates notes jumping about. I do not care about what I am seeing outside, I am occupied with myself. My ego has changed, I feel only a skeleton, the flesh and the accessories have apparently vanished from me."

11.48. "This is a low form of escape, because everything comes back, after all. It is rather tiresome to feel that one is not identical with oneself and that one is unable properly to express his ideas." (Listening to Don Juan by Strauss): "This does good to one's feelings, it appears to make sense, as if it had a shape."

12.05. "All is well, all is well as it is, we lower the standards not for ourselves, but for the environment." (While walking): "As if I were not walking, but the walls would be coming closer to me. It would be good if this would never end, because it is merely a registration into a kind of mood, thus, nothing happens: this is a high form of entertainment, that I close my eyes and think about what is happening, and so on, this, too, is like that, meanwhile I am waiting and waiting, there are also difficulties in communicating my thoughts, as if the text had nothing to do with me."

12.12. "As if the essence of everything were brought to the foreground. If I 'ego-ize' my hand, it is big. My body is a bit strange from the neck downward."

12.16. Pulse rate 88/min. BP: 190/110 mm. Hg. "It is now for the first time that I know convention to be merely resistance brought to the consciousness. The relation of personality to communication is a grand thing: I start an idea in my mind and I say almost the opposite of it. Words are no longer suitable for expressing my thoughts. Also the world around me has changed: everything has a purpose, things have been distorted in the direction of their essence, what is big is very big, what is small is very small, and so forth."

12.23. He keeps hearing bells ringing, Easter bells. He asks for no music, because he hears music, ringing of bells, organ music, etc. even without the gramophone.

12.28. Asks for a looking-glass: "I do not let even facts influence me, I am pleased with myself."

12.31. The room appears to be big, as if it could hold everything, the walls are far away.

"If we could inoculate this into all men, human inter-relations would undoubtedly improve greatly."

12.37. "Works of art have lost their significance, anything others have made is superfluous."

"I am feeling fine, I do not care even about my afternoon programme, everything is so far from me. Production is independent from its ethical charge, thus the problem of deceit—no deceit is unimportant."

12.46. "I miss only one thing, the joy of productivity. I am kind of depressed, I have not promoted my cause, nor that of others, for that matter. To a certain extent it is depressing that I am irresponsible, this is the '*sapienti sat*' problem essential also for modern art."

12.55. Pulse rate 88/min., BP: 175/100 mm. Hg.

He is euphoric, but slightly tired. He sees colours too brightly, believes a painting to be a relief.

Owing to his elevated blood pressure this test subject had been given a smaller dose of DET and thus his response was milder. None the less, loosened associations, the inclination to philosophic thoughts, the drive to speak, the appearance of new modes of experience, the difficulty of wording (he is an author-aesthetician!) were readily observable features, whereas the impairment of perception was less marked.

The symptomatology of the DET model psychosis may be summarized as follows.

(1) Vegetative symptoms. These appear 8 to 15 minutes after injection and are: vertigo, dilatation of the pupil, nausea, sweating, tremor, increase in pulse rate and blood pressure (to 120/min. and by 10 to 40 mm. Hg. respectively). The subjective complaints similar to a "hangover" are closely

similar to what occurs in the initial phase of the mescaline effect, but most of the unpleasant symptoms subsided within 40 minutes.

(2) The neurological symptoms were less marked in the DET effect than in the response to DMT. The main complaint was paresthesia, whereas transient increase and differences in reflexes, ataxia of the extremities and hypalgesia of the whole body occurred in some cases only. Some subjects experienced an increased drive to motor activity, sometimes in connection with anxiety. The athetoid motions of the left hand described by Szára have not occurred in our cases. We mention here also the impairment of the ability of drawing, though in some cases the distortion of perception and the mixed ataxic-apraxic disturbance undoubtedly played a role in it. A good example for this is a schizophrenic painter, who drew her own left hand in the different phases of the effect. In her case metamorphopsia, too, had undoubtedly a role to play (Fig. 1). The two drawings before experiment show clearly the



FIG. 1

excellent technique of the professional painter (a). In the drawings begun at 60 and 100 minutes, respectively (b) the lines are loosened, uncertain. This is even more marked in the drawing started at 120 minutes (c), showing a distortion of shapes, of which she, too, complained. The drawings started 3 hours after the injection of DET (d) are comparable in quality to the pre-test ones. It is also remarkable how the artist ignored the quality of her performance during the experiment.

(3) Just as in the LSD and mescaline effects, the predominant disturbances of perception are visual ones under the influence of DET. Visions of elementary nature, abstract patterns, arabesques, etc. were common, others told us about scenic pictures ("a scene of ballet", "ornamental fish swimming to and fro", "dance of Centaurs", etc.). All these were essentially pseudo-hallucinations. Most of the test subjects had an illusionistic view of the surroundings ("everything looks like scenery", "the faces are like caricatures", "primitive", "of the Eastern type".)

Also the perception of space was often disturbed. The planes, outlines were distorted, the walls have shifted away, curving slightly, some objects seemed to come menacingly nearer and nearer. The hallucinations were mainly colourful. It is remarkable that with the eyes open the visions and illusions had a shade of blue in the purple, whereas with the eyes closed the red and orange colours dominated.

Auditory hallucinations occurred less frequently, hyperacusis was more common. Music had a remarkably pleasant effect. In many cases music started synaesthetic experiences, as it was characteristically told by one of the test subjects, for example. He was listening to a violin concerto by Kabalevsky and said:

"The materialization of sounds varies all the time. Once ballet dancers dressed in transparent silver-like clothes were dancing, sliding on ice, then blades, lances, swords of different shapes broke through space, twinkling, shining, turning round and round. Then cliffs split apart, snow-covered peaks and blue ridges emerged from the ground toward the sky, then again there were many small ladders of glass and on them minute globules of sound were running up and down, dancing and jumping. Then again a small goldfish swam rapidly across an aquarium and the sound was the row of bubbles in its wake."

(4) In the majority of the normal test subjects the perception of time was definitely disturbed. Some of them spoke of a pleasant feeling of "timelessness", of a "loss of the category of time".

(5) Associations were usually accelerated, loosened, whimsical. During the second half of the experiment introversive or philosophic tendencies preponderated. In schizophrenia DET increased pre-existent changes in thinking, though in many a case this did not become manifest owing to the development of autism.

In 12 cases (non-psychotic test subjects) a shortened thematic apperception test was carried out 1 hour after the injection of DET, showing 3 pictures to each subject. (Nos. 1, 12 and 14.) Usually they did not concentrate on the task, only two test subjects told appropriate stories, but even these were conventional, bringing to surface nothing important. The others described the pictures grossly, some subjects criticized the quality of the pictures.

For example: "I have nothing to relate, at the most one might tell a

sentimental story, but I do not feel like doing that! I do not feel myself touched at all. A child leaning over a violin, a rotten picture."

(6) Mood was usually heightened, to euphoria except in 4 test subjects. Transient anxiety was common, usually with increased motor activity.

Many test subjects told us about having experiences of the "charismatic" nature (Savage and Cholden). They felt like approaching "truth", like getting ultimately a true picture of the world ("things have shifted toward their essence", "objects become their own symbols", etc.). These experiences further heightened mood.

(7) In the majority of the cases consciousness was clouded, and most test subjects likened the experience to drunkenness or to being half asleep. Every normal test subject remained contactible. Episodes of depersonalization phenomena, a strange realization of motor activity, uncertainty of body scheme, etc. marked the mild disturbance of the ego-consciousness. The so-called double orientation, which makes self-analysis, concentration on the pathological experience, but at the same time contact with the environment possible, was part of the DET effect, like it was with the other psychotogens. Otherwise, the clarity of consciousness varied, with its perimeter narrowing and widening.

(8) The eventual humoral shock-effect of DET has been studied in 10 test subjects, but all that the 0.8 mg./Kg. dose produced was a moderate lymphopenia and variations in the serum K level.

(9) The immediate after-effects of DET were slight fatigue, mild headache, depression and insomnia, respectively, which, however, did not exceed one day in duration.

It should be pointed out as a peculiar late effect that many of the normal experimental subjects showed artistic tendencies which they had not shown before. The majority of them, when relating their experiences, were sometimes subjective and rather lyric at times. This is understandable. But two subjects began painting pictures shortly after the experiment, one wrote a poem and another a short story, although none of them showed such tendencies before. To explain this sudden outburst of creative tendencies the same possibilities may be brought up as those mentioned by Arnold in his study on the creative activity in the early phase of schizophrenia. Of course, subconscious contents may be activated in this way, with the energy associated with them, but it is also possible that the work of art thus created is the result of a defensive reaction, initiated by the *ab ovo* imbalanced personality which suffered a trauma during the experiment. One of the test subjects began to paint "to be able better to express his otherwise inexpressible experiences and to explain them to his friends".

(10) EEG observations.

In 31 cases intoxication was followed up by EEG studies. Owing to a lack of co-operation, the electrograms for 6 psychotic females cannot be evaluated and thus an account can be given of the EEG results for 10 normal adults, 14 schizophrenics and 1 female with epilepsy.

The acceleration of rhythm and/or the decrease of amplitude are detectable in the curves of all the 25 subjects examined. The acceleration of the alpha rhythm amounted in general to 1 to 2 c/s, in 2 cases it was 3 c/s. In 2 curves the initial rhythmic theta component disappeared at the peak of the DET effect and the alpha component was moderately intensified. (One

If these patients had been leucotomized 9 years earlier because of schizophrenia, the other was a female suffering from epilepsy.) The alpha-desynchronization on opening the eyes was complete even in the spontaneous curves in 10 subjects, it became complete in response to DET in 13 and remained partial in 2 cases. The amplitude and regularity of the alpha rhythm decreased in 23 cases, of which in 5 a transient full desynchronization developed (Fig. 2). A paradox alpha reaction did not occur. In 2 cases the

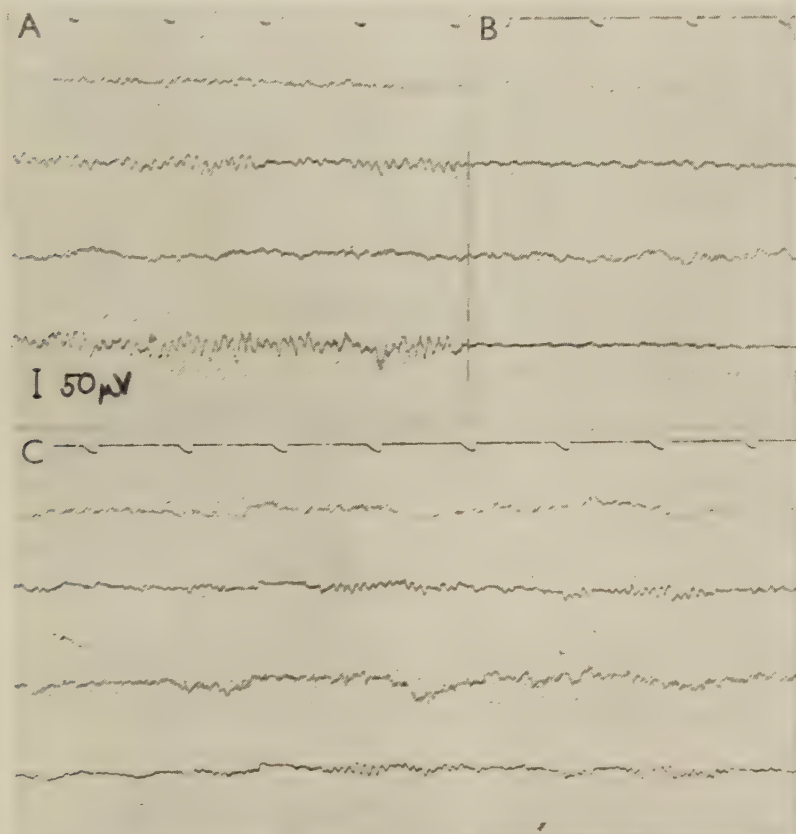


FIG. 2.—Female, aged 25 years. (a) Spontaneous EEG. (b) 55 minutes after the intramuscular injection of 0.8 mg./Kg. DET. (c) 85 minutes after injection.

All three records have been made with the patient's eyes closed. (Channel 1: right frontoprecentral, 2: right precentro-parietal, 3: left fronto-precentral, 4: left precentro-parietal. Time: 1 sec. Standardization: 50 μ V.)

alpha amplitude increased, but at the same time the alpha frequency increased.

In general, the EEG changes appeared parallel with the clinical development of the response to DET, with the maximum effect occurring 50 to 80 minutes after injection in the majority of cases. There was usually a marked parallelism also between the clinical symptoms and the absolute intensity of EEG changes. No correlation was observable between the single clinical symptoms (visions) and the EEG. In a case of a female schizophrenic the changes of the EEG differed somewhat from what was seen in the other cases. The amplitude of the alpha rhythm was markedly reduced and at the peak of the DET effect a 6 to 7 c/s high amplitude, bilateral-synchronous theta rhythm

appeared in the anterior leads, which could be only suspected to be present in the spontaneous curve. This patient had been given 0.75 mg./Kg. of DET and clinically the intoxication was very mild.

The EEGs of the two hemispheres behaved in the same way in every case. There was no difference in the changes between the normal subjects and the psychotic ones, either. In the patient with epilepsy the focal left precentral steep-slow complex of the EEG did not change at the time DET was acting, though otherwise the wave spectrum was accelerated.

Thus, it is clear that DET, like mescaline, LSD, DMT and bufotenine, arouses electrical alertness.

As to the mode of action of DET, we refer to the observations made by Purpura, Rinaldi and Himwich, Marrazzi and Hart with LSD and by Kajtor with DMT and we assume that also that agent has a dual excitatory and inhibitory action, involving a facilitatory effect on the specific projection pathways and an inhibition of the cortico-cortical, transcallosal and of the non-specific projection systems. However, the clinical picture of the DET effect suggests that a dissociation (disturbances of somato-sensibility) may occur within the action on the specific afferent system, and certain forms and colours of the subjective visual phenomena indicate that there may be a direct action on the retinal synapses as well.

The majority of the uncommunicative, chronic schizophrenics became more communicative in response to treatment with DET. Even the vegetative symptoms and disturbances of perception did not interfere with the improved contact and the patients were particularly communicative during the third hour of the DET effect. Likewise, a favourable change followed the DET effect in 4 psychopathic and 3 severely hysterical patients. In cases of psychosis and neurosis DET appears to be superior to the intravenous barbiturates for exploration and for establishing psychotherapeutic contact, because the peculiar dual state of inhibition-excitation induces them to tell the truth about themselves. Changes in the mood, the temporary disinhibition, the increase in the intensity of experience, together with the possibility of acting out have a role in therapy. In the following we present a few illustrative examples of this.

Mrs. J.W., aged 35, had been suffering from depressive schizophrenia for 3 years. In response to DET she developed vivid visual hallucinations and illusions, then told us that she was still yearning for her lover of 14 years ago, who was living far away. It was because of him that she had divorced her husband and did not want to get married again. She was waiting for a miracle to bring her together with her lover again. (Insulin coma and other active treatments continued over months had failed to bring any of these ideas to the surface.)

H.E., female, aged 38, paranoid schizophrenic for 6 years, told us for the first time under the DET effect that she was strongly bound emotionally to her father. She did not get married, but established instead extramarital relations with a man resembling her father. The wife of that man had tuberculosis and she often wished that the wife would die and was therefore tortured by bad conscience. She would not be in that situation, were her father still alive.

L.M., female, aged 29 years, had been suffering from catatonic schizophrenia for 5 years. Under the influence of DET she became talkative and spoke about love of her fellow men. Later she told us that she had been attracted by one man only, a priest, and she would like to write to him. People were bad, because they gossiped about her, telling that she had been in bodily contact with that priest.

Gv. J., female, aged 25 years, with acute paranoid reaction, acted out catharsis-like her conflict (she had a love affair with a married man and was afraid that her parents would discover it) under the DET effect and became symptom-free afterwards. Later, a relapse occurred at home due to financial and familial difficulties. Then she asked for DET again, because she felt that it had done good to her in the past.

Many other patients with psychopathy told us truthfully about their problems under the influence of DET, but many protested against being given another injection, because they had talked too much under the DET effect.

The most suitable dose appears to be 0.8 mg./Kg. Whether it should be repeated or not, remains to be elucidated; we have given repeated injections to 5 patients only. In schizophrenics showing a tendency to stupor and in a state of excitation DET cannot replace, of course, shock and narcoleptic treatments, respectively and in such cases the administration of DET is unreasonable. Experience has shown us thus far that on completion of active treatment, at the beginning of socialization trials we can expect the most benefit from DET. A DET-interview may help to unmask eventual tendencies to dissimulation. Confabulation, so common in barbiturate exploration, did not occur under the DET effect, which seems to incline the patient to be frank and to seek support; it happened only in 2 cases of paranoid schizophrenia that the patients thought of poisoning and turned hostile to the experimenter.

DISCUSSION

DET appears to be highly suitable for medical autoanalysis, particularly because it exposes the body to less stress than LSD or mescaline; its effect is over in 3 hours and yet it made a temporary "living through" of many pathopsychological symptoms possible. The euphoria the drug produces may have the danger of addiction, but thus far only a single normal test subject has expressed a desire to have the DET model psychosis repeated.

Although the number of non-schizophrenic patients involved is small, also some of the so-called "normal" test subjects exhibited neurotic and psychopathic features. Experience has shown DET intoxication to be suitable for studying personality components and psychic features, because they become more plastic during experiment. In connection with the DET effect we can confirm Becker's statement for the LSD experiments, according to which the content of manifestations depends on the life story, but constitutional factors also play a role in the evolution of a syndrome, some of which may have a special trend of manifestation that may be provoked equally by a toxic or a reactive psychogenic effect.

For obvious reasons, we did not want to explore our normal test subjects who had volunteered for the test, but even so they revealed much about themselves; even when relating later the experience they often spoke about data directly pertinent to their problems, desires and failures. We did not analyse directly their visions, although even these, being so similar in quality to day-dreaming, appear to have brought to surface ego-near contents, with a katathym-effective background, or wishfully, or reflecting fears (eventually symbolically). For example, one of our novelist-authors claimed that his nervous tension he called "cosmic anxiety" disappeared only for the 3 hours of the DET effect during some years; during the DET effect he could think over his artistic problems better. Another neurotic experimental subject, who is deeply religious, spoke about a union with God he had lived through ecstatically during the DET effect, after which he was extremely happy for weeks, because, as he put it, the DET effect proved his religiousness to be profoundly sincere and not only apparent.

Thus, DET may prove to be a useful aid in the psychotherapy of certain neurotics as well and the contents thus explored may be utilized also in psychoanalysis. The resulting regression, loosening of consciousness and the

desire for help increase suggestibility, help to establish psychotherapeutic contact, and to resolve for a time the progressive narrowing of life experiences, which, according to Lewis and Sloane, is one of the principal features also in the therapeutic effect of LSD in neurosis.

Several test subjects likened the DET effect to the delirant state of typhus, pneumonia, trauma, etc., thus it has been suggested that the tryptamine derivatives may play a role in the genesis of certain exogenous pathological conditions. The severe disturbance of consciousness, which occurs as a rule in such conditions, and which greatly interferes with objective remembrance, may be explained by the presence of excessive amounts of toxic substances or by a loss of compensatory power in the body. In support of our view is the claim made by Turner *et al.* that "any psychic action of bufotenine must be of a delirious nature, i.e., of a toxic interference with cerebral mechanisms", as well as the statement made by Fabing and Hawkins in which the potential role of anoxia in the development of the bufotenine syndrome is emphasized. Maybe the "Zwischenstoffe" claimed to exist by Bonhoeffer are unmasked as metabolites of tryptophane. Further experiments with similar agents may contribute to our knowledge of the pathogenesis of the exogenous reaction types.

Research into the experimental psychoses involves innumerable methodological difficulties. These difficulties surpass even those faced in clinical pharmacology, if the complete symptomatology of the new psychotic agents should be described. In such a case namely the classic method of psychiatric observation and the subjective data supplied by the test subject should be relied upon, in the first place, the EEG and the various tests and repeated blood samples all interfere with the results, although the data obtained facilitate a more objective comparative study of the effect. The personality of the test subject and the pre-experimental atmosphere may have a decisive influence on experience and the repeated test with the same agent will find a biased test subject. It would be very difficult to apply the placebo or double-blind methods in the control of the experiments and the suggestive influence of the test subjects on one another is also very difficult to eliminate. The differences in response due to differences in personality can be elucidated only by carrying out very many tests and careful, detailed personality studies before the experiment. These evaluative difficulties were evident also by our DET experiments.

Although it is mainly in the field of phenomenology that our investigations are complete, it seemed worth while to outline the properties of this new tryptamine derivative, particularly because we believe DET to be the best and least noxious psychotogenic agent known thus far, which seems to have an unquestionable therapeutic effect as well, as is illustrated by the examples described.

SUMMARY

After discussing the hallucinogenic derivatives of tryptamine known thus far and their significance, the authors outline the symptoms of the model psychosis which followed the intramuscular injection of a single dose of 0.65 to 0.85 mg./Kg. body weight of diethyltryptamine. A total of 71 test subjects (30 normal, 41 psychotics including 29 schizophrenics) have been involved in the study. The model psychosis produced by DET can be best likened to a moderate mescaline intoxication, although in some features it resembles the LSD effect. The effect is over in 3 hours. An illustrative protocol is presented and examples showing how the drug may help in psychotherapy are discussed in brief. The EEG changes produced by diethyltryptamine are described shortly; these bear resemblance to those produced by mescaline and LSD.

It is suggested that a correlation may exist between the tryptamine derivatives and the exogenous reaction types of mental condition.

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THE ASSESSMENT OF SOME SYMPTOMS AND SIGNS OF DEPRESSION IN WOMEN

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INTRODUCTION

THE word "depression" has come to be used in psychiatry to denote both a syndrome (or symptom:sign cluster) and one or more of the symptoms commonly occurring in this cluster. This attribution of two meanings to one term has led to considerable confusion. Thus it is often claimed that depression may be present in almost any mental illness. This statement may be true in the sense that the mood of depression may occur in almost any mental illness; but it cannot be true in the sense that the depressive syndrome may enter into almost any mental illness, since the term depressive syndrome has already, at least by implication, been used to denote a particular symptom:sign cluster which differentiates it from all other symptom:sign clusters. To deny this would be to deny the original principles of division.

Clinical judgment involves the inevitably selective recall of tentative, personal and relatively incommunicable, symptom norms. It is the capacity to educe relevant relations between the presenting cues, including subliminal ones, and these personal norms that underlies the clinical art. Where there is so much dependence on selective recall the pitfalls are numerous and vast. One of the more subtly concealed pitfalls may be a tendency to recall more readily those symptoms having a high degree of commonality at the expense of those, perhaps relatively few, which have a high degree of specificity. This view is itself subject to the dangers implicit in selective recall and it may be that it merely illustrates the propensity, recently attributed to a well-known politician, for preparing pits with unusual care before falling into them. It would seem important, however, to try to determine which "depressive" symptoms or signs are more or less equally common among depressive and non-depressive syndromes and which are significantly more frequent among depressive than non-depressive syndromes. The same considerations can then be applied to psychotic as opposed to psychoneurotic depressives.

Before going on to consider the differentiation between psychoneurotic depressives and melancholics, it is perhaps worth while to examine the logic of the arguments against making this split. The arguments advanced today are still the same as those which Lewis (1934) reviewed almost a quarter of a century ago. Bumke's statement (1929), quoted by Lewis, may be taken to represent the first position. "There is such a mixing-up of the endogenous forms that it is often quite arbitrary whether one speaks of mere nervousness or of manic-depressive psychosis or hysteria . . . To this extent the not infrequent question as to whether there is a fundamental distinction between mild

melancholia and neurotic depression seems to me a false one (or wrongly posited)."

Bumke has taken one alleged attribute of a psychosis—namely its endogenous nature—and, by implication, one alleged attribute of neurosis—namely its exogenous nature—and argued that, since these attributes are so mixed up as to be virtually inseparable, no fundamental distinction between psychosis and neurosis can be made. This is analogous to first defining women as the ones who dance backwards, then discovering that everyone who dances sometimes dances forwards and sometimes backwards, and concluding from this that sex differences do not exist, or at least have no pragmatic value. The distinction between endogenous and exogenous was always too difficult to provide a useful basis for separating psychotics from psychoneurotics; but it does not follow from this that the separation cannot be made on the basis of other criteria.

Adolf Meyer's view (Meyer, 1922), also quoted by Lewis, that no sharp line can be drawn between the psychoses and the neuroses because many neuroses represent a more far-reaching disturbance in the general adjustive equilibrium of the individual than the psychoses, provides a slightly different argument. Clearly Meyer has been able to identify psychotics and neurotics by means of some attribute, or attributes, other than the extent of "disturbance in the general adjustive equilibrium of the individual" (itself a remarkably untestable hypothesis). Had he not been able to do so he could not have concluded that they did not differ in this particular attribute. All that can safely be established from his statement is that all psychotics do not differ from all neurotics in all respects. For pragmatic purposes, all that we need to establish is that most psychotics differ from most neurotics in some respects. "All" has to be reduced to "most" because none of our measures of human attributes is, or is likely to be, free from various sources of error.

To develop a useful classificatory system we do not need to draw a sharp, indelible line. We can set up criteria for psychosis and neurosis (quite other than endogenous and exogenous) and we can ask competent draughtsmen to draw tentative lines and we can see whether or not they draw them in approximately the same place. Further, we can decide whether the approximation is good enough for our purposes.

That a research worker should take a more benign view of nosology than the clinician himself takes is perhaps not surprising. Ideally the research worker looks at a more or less random sample of a group and sees the ratio between typical and atypical cases; the clinician, on the other hand, when drawing on his clinical experience, is liable to be selective in his recall. The difficult diagnostic problems, which have been presented at case conferences or which have occasioned disagreement with colleagues, will tend to be recalled; whereas, the straightforward cases, about which little discussion is needed, will tend to be overlooked. However this may be, in a previous investigation (Foulds, 1955) in which 36 patients were diagnosed within a week of admission to Runwell Hospital independently by two experienced psychiatrists, agreement was reached in respect of the psychotic:neurotic classification in 30 of these cases. The association between the judgments was significant at the 0.1 per cent. level. The psychiatrists disagreed in 2 out of 8 cases involving the question of psychotic or neurotic depression. This, as far as it goes, is not too discouraging, particularly in view of the short period of observation.

The aims of the present investigation were to try (i) to distinguish on psychological tests between depressives and non-depressives and (ii) to differentiate psychotic from psychoneurotic depressives.

SUBJECTS

Sixty women, admitted to Runwell Hospital and equally divided between the four diagnostic categories of Hysterics (representing a non-depressive psychoneurotic group), Paranoid States (stretching to Paranoid Schizophrenia and representing a non-depressive psychotic group), Psychoneurotic Depressives and Psychotic Depressives (referred to for ease of distinction as Melancholics).

TESTS

The test scales used as possible measures of symptoms were the Depression Scale of the Minnesota Multiphasic Personality Inventory and three new scales derived from this inventory and called Self-Criticism, Guilt and Manifest Hysteria.

The mean total words per picture on the Thematic Apperception Test and the distraction effect on the Mazes were utilized as "sign" measures, since these had differentiated between Hysterics and Dysthymics in a previous study (Foulds and Caine, 1958). The sign measures and the Manifest Hysteria Scale were given to the Hysterics and Depressive groups only.

M.M.P.I. Depression Scale: McCall's (McCall, 1958) breakdown of the Depression Scale into items having face-validity for, apparent congruence with, and apparent irrelevance to, depression was utilized. A further breakdown into affective, functional and health items was made with seven resulting subdivisions: Affective face-validity (Afv); Functional face-validity (Ffv); Health face-validity (Hfv); Affective congruent (Ac); Functional congruent (Fc); Health congruent (Hc) and Irrelevant. The numbers of the M.M.P.I. items are given in the Appendix, but three examples of each were as follows:

- Afv: F36 wishing I could be as happy as most others; F38 not feeling that life is worth while; F39 not finding daily life interesting.
- Ffv: B2 not understanding what I read as well as formerly; C17 not being as able to work as ever; F42 having difficulty in starting things.
- Hfv: A2 not being in as good physical health as friends; A6 worrying about health; B18 troubled by constipation.
- Ac: A8 believing I am more nervous than most; C48 not liking to flirt; G31 never feeling like smashing things.
- Fc: A27 memory not seeming all right; B31 being easily awakened by noise; E36 not being a good mixer.
- Hc: A1 not being well most of the time during the last few years; A4 either gaining or losing weight; A40 often feeling weak all over.
- Irr: A15 being troubled by attacks of nausea and vomiting; C33 not agreeing that people are hard to convince of the truth; D22 not believing things are turning out as predicted by the Biblical prophets.

Self-Criticism and Guilt Scales: Twenty-eight items were drawn from the M.M.P.I. which it was thought implied self-criticism in varying degrees and from this list 10 items were selected which it was thought would separate psychotic from psychoneurotic depressives. Our practice has been to exclude from this excessively long test all those items which do not enter into any of the original scales. Unfortunately this resulted in the exclusion of 7 of the 28 items considered suitable as measures of self-criticism and 3 of these were among the 10 especially selected to pick out psychotic depressives. The Self-

Criticism Scale is for the present, therefore, reduced to 14 items and the Guilt Scale to 7 items. Examples of the Self-Criticism Scale are as follows:

A23 not being as capable as most others; C29 not usually expecting to succeed in things I do; I41 shrinking from facing a crisis or difficulty.

The Guilt items are given in full below.

Manifest Hysteria Scale: These items are given in full below.

T.A.T.: Total Words: This measure was the mean number of words per picture using 3F, 6F, 9F and 13MF.

Maze Distraction: This latter score is the total time taken to carry out a second run-through of the mazes, in which the subject repeats numbers after the tester, expressed as a percentage of the time taken on the first run-through, in which there is no counting.

RESULTS

Table I shows the means and standard deviations for Hysterics, Paranoid States, Depressives and Melancholics on the full Depression Scale, on each of the sub-groups of this scale, on the Self-Criticism and Guilt Scales. Table II shows the significant t and p values derived from Table I.

TABLE I

The Depression, Self-Criticism and Guilt Scores of 15 Female Hysterics, 15 Female Paranoid States, 15 Female Depressives and 15 Female Melancholics

	Hysterics		Paranoids		Depressives		Melancholics	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
D scale ..	32.93	5.00	22.73	10.89	34.27	7.92	34.60	5.73
Afv ..	6.47	3.30	3.80	2.44	7.53	3.22	8.47	2.33
Ffv ..	4.07	1.69	1.87	1.67	4.27	2.14	5.27	1.24
Hfv ..	3.53	0.88	1.60	1.31	3.07	1.44	2.87	1.09
Ac ..	7.67	2.41	7.13	2.13	8.47	2.12	7.93	1.95
Fc ..	2.13	1.41	1.40	0.66	2.53	0.81	1.87	1.02
Hc ..	1.93	0.93	0.67	0.70	1.60	0.80	1.13	0.81
Irr ..	7.13	1.31	6.27	1.53	6.80	1.91	6.60	1.38
Afv+Ffv	10.53	4.56	5.67	3.61	11.80	5.20	13.73	2.98
S-C ..	8.33	4.41	4.47	3.44	9.00	3.39	8.73	2.69
Guilt ..	2.00	1.59	2.07	1.48	1.73	1.69	4.13	1.59

D Scale=Raw scores for the full M.M.P.I.: D scale; A=Affective; F=Functional; H=Health; Irr.=Irrelevant; fv=face validity; c=congruent.

TABLE II

Significant t and p Values Derived from Table I

	M : H		D : H		M : P		D : P		H : P		M : D	
	p<	t	p<	t	p<	t	p<	t	p<	t	p<	t
D scale ..	—	—	—	—	6.71	.001	4.83	.001	5.87	.001	—	—
Afv ..	—	—	—	—	5.22	.001	3.47	.01	2.45	.05	—	—
Ffv ..	2.14	.05	—	—	6.13	.001	3.31	.01	3.46	.01	—	—
Hfv ..	—	—	—	—	2.80	.01	2.83	.01	4.58	.001	—	—
Ac ..	—	—	—	—	—	—	—	—	—	—	—	—
Fc ..	—	—	—	—	—	—	3.94	.001	—	—	—	—
Hc ..	2.44	.02*	—	—	—	—	3.28	.01	4.06	.001	—	—
Irr ..	—	—	—	—	—	—	—	—	—	—	—	—
Afv+Ffv	2.20	.05	—	—	6.45	.001	3.62	.01	3.13	.01	—	—
S-C ..	—	—	—	—	3.69	.001	3.51	.01	2.58	.02	—	—
Guilt ..	3.57	.01	—	—	3.53	.01	—	—	—	—	3.89	.001

M=Melancholics; H=Hysterics; D=Depressives; P=Paranoids.
* In the "wrong" direction, i.e. H scored higher than M.

The Depression Scale: It can be seen that Melancholics are distinguished from Hysterics on three measures—Functional face-validity, Affective together

with Functional face-validity and Health congruent (but this last in the wrong direction, i.e. Hysterics scored higher).

Melancholics can be distinguished from Paranoid States on the full D Scale and all face-validity measures, but not on any of the congruent or irrelevant measures.

Depressives can be differentiated from Paranoid States on all measures except Affective-congruent and Irrelevant.

For these symptoms to be regarded as specifically depressive—whether psychotic or psychoneurotic—Melancholics and Depressives on the one hand must each be distinguishable from Paranoid States and Hysterics on the other. Depressives cannot, however, be distinguished from Hysterics on any of the measures. It must, therefore, be concluded that even those symptoms having face-validity for depression occur with indistinguishable frequency in at least one depressive and one non-depressive syndrome. These results support the view that complaints of depression enters into non-depressive syndromes.

Face-validity measures differentiate significantly, and in the right direction, between depressive and non-depressive subjects in 7 instances; congruent measures differentiate between these groups in only 3 instances (and one of these in the wrong direction). Irrelevant measures do not differentiate at all. The value of McCall's categorization is, therefore, supported by these results.

The Self-Criticism Scale: behaves in very much the same way as the full Depression Scale. The only point of interest here is that Paranoid patients are not inclined to be self-critical even over relatively trivial matters.

The Guilt Scale: In the present investigation the psychotic:neurotic depression split was made reluctantly by some psychiatrists and without setting up agreed criteria beforehand. Nevertheless the resulting two groups were shown to differ very markedly on a simple measure which it was predicted would do just that. The 7 items of the Guilt Scale each differentiated in the predicted direction between Melancholics and Depressives. Ten Melancholics to 1 Depressive said: "I wish I could get over worrying about things I have said that may have injured other people's feelings"; 12 Melancholics to 2 Depressives said: "At times I think I am no good at all"; 8 to 3 "I have not lived the right kind of life"; 7 to 3 "I believe my sins are unpardonable"; 6 to 5 "I believe I am a condemned person"; 9 to 5 "Much of the time I feel I have done something wrong or evil"; and 15 to 9 "I certainly feel useless at times".

The Guilt Scale, even with only 7 items, differentiates Melancholics from Hysterics and from Paranoid States almost at the 0.1 per cent. level of significance and from Depressives at beyond this level. Ten out of 15 Melancholics admit to 4 or more of the 7 items against 2 Paranoids, 2 Hysterics and 1 Depressive. Since each item in the scale was selected to differentiate in the way in which it did in fact do so, the scale is unlikely to have benefited unduly from chance differences in a favourable direction. McCall's analysis into face-validity, congruent and irrelevant items of the Depression Scale and his finding, confirmed here, that differentiation between depressives and non-depressives falls off in that order, suggests that the M.M.P.I. is not immune to this criticism.

Manifest Hysteria Scale: One differentiation remains to be made, that between Hysterics and Depressives. Ten items, having face-validity for Hysteria, differentiated these groups at the 0.1 per cent. level of significance (t being 5.07). Hysterics had a mean score of 5.73 (s.d. 1.53) against 2.47 (s.d. 1.86)

for Depressives. In fact all 15 Hysterics answered 4 or more of the 10 items in the scoreable direction against 3 Depressives. The details were: 13 Hysterics to 5 Depressives said they have not been well mostly (A1); 12 to 8 said that they worry about their health (A6); 10 to 5 that they are not in as good health as friends (A2); 9 to 4 that they have been paralysed or had weakness in their muscles (A41); 8 to 5 that they have burning, tingling, etc., sensations (A47); 3 to 4 that they have often had stomach trouble (B15); 8 to 1 that they have had heart or chest pains (B9); 7 to 3 that they have had numbness of the skin (A48); 7 to 3 that they have had blank spells, etc. (A21); and 4 to 1 that they have had uncontrollable fits of laughing and crying (A22).

It would appear, therefore, that Hysterics complain of depressive symptoms, but that Depressives do not complain of hysterical symptoms. There is an insignificant correlation ($r = .212$, $p > .05$) between the face-validity Hysterics items on the one hand and the face-validity affective and functional items of the D Scale on the other.

"Sign" results: The next question to consider is whether the depressive symptoms complained of by Depressives and Hysterics alike affect their behaviour equally. Do the signs go with the symptoms? Table III shows the

TABLE III

<i>T.A.T. Productivity and Maze Distraction Score of 15 Depressives and 15 Hysterics</i>						
		Depressives		Hysterics		
		Mean	S.D.	Mean	S.D.	t
						p
T.A.T.	..	76.40	28.78	98.00	25.33	2.11
Mazes	..	62.87	17.90	74.60	22.74	1.52
						< .05
						> .1

means and standard deviations for these two groups, together with the t and p values for the differences.

Hysterics are significantly more productive on the T.A.T. than Depressives and they speed up less on the mazes with distraction. This second difference does not, however, reach an acceptable level of significance; but it is of interest that the face-validity Hysterics Scale correlates significantly with the Maze Distraction Scores ($r = .409$, $p < .05$). A further point of interest is that the T.A.T. measure correlated (ρ) $-.253$ with Affective and Functional face-validity within the Depressive group and $+.264$ within the Hysterics group. Although the difference between these correlations was not significant, there was a tendency for the Depressives with the higher "Depressive" scores to be less productive on the T.A.T.; whereas the Hysterics with higher "Depressive" scores tended to be more productive.

The evidence from these sign measures is perhaps sufficient to suggest that the "depressive" complaints of Hysterics may be more in the nature of self-dramatization than self-description. These "sign" results should not be regarded as confirmatory of those obtained in an earlier study (Foulds and Caine, 1958) since there was some overlap in the subjects used.

DISCUSSION

Confirmation of McCall's results, together with some additional findings, leads to the belief that the empirical approach of the M.M.P.I. may be less rewarding than one tied more closely to psychiatric experience. An attempt should be made to construct a symptom check-list based on the following

principles: (1) that one should begin with items having agreed face-validity; (2) that one should endeavour to use, as far as possible, items likely to have a high degree of specificity rather than generality; (3) that, and this would follow from (2), the number of items should be drastically reduced; (4) that items should be symptoms and not personality traits. Almost all previous scales have confounded the two types of item.

CONCLUSIONS

In an investigation, involving 60 female patients, 15 of whom were psychotic depressives (Melancholics), 15 psychoneurotic depressives (Depressives), 15 non-depressive psychotics (Paranoid States) and 15 non-depressive psychoneurotics (Hysterics), it was found that:

1. The Depression Scale of the Minnesota Multiphasic Personality Inventory differentiated Paranoid States from each of the other groups, but not between these other groups.

2. McCall's finding—that items chosen from the D Scale for face-validity differentiated better than items which were merely congruent with depression and that these differentiated better than those which, clinically, appeared to be irrelevant—was confirmed.

3. Affective and Functional face-validity items together added to the differentiations obtained with the D Scale that of Melancholics from Hysterics.

4. A new Self-Criticism Scale behaved in very much the same way as the D Scale.

5. A new Guilt Scale (composed mainly of delusional items) differentiated Melancholics from all other groups, including psychoneurotic Depressives.

6. A new face-validity Hysteria Scale differentiated Hysterics from Depressives. These groups were also differentiated on a "sign" test—Productivity on the Thematic Apperception Test.

7. It was thought that the results suggested the need for a Symptom Inventory rather than an inventory in which symptoms and personality traits are inextricably confused.

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APPENDIX

The numbers of the M.M.P.I. items are as follows:

Affective face-validity: B30; E41; F36; F38; F39; F45; F51; G7; G12; G18; G25; 137; 139.

Functional face-validity: A23; A24; B2; C17; F42; F44; G23; I27.

Health face-validity: A2; A3; A6; B12; B18.

Affective congruent: A8; B36; C48; D49; E24; F34; G31; G32; G38; H43; H55; I34; I50; J51.

Functional congruent: A27; B28; B31; E36.

Health congruent: A1; A4; A40.

Irrelevant: A15; A18; B3; B4; B6; B8; C31; C33; D10; D18; D22; G37; I4.

Self-Criticism: A23; C25; C29; F4; F5; F6; F33; I13; I25; I26; I31; I39; I40; I41, and the following items selected, but inadvertently omitted: B44; B45; C19; F32.

Guilt: F50; G3; G4; G9; I17; I36; I37, and the following items selected, but omitted: F40; G5; G8.

A COMPARISON OF THE PSYCHOLOGICAL EFFECTS OF ACUTE AND CHRONIC ADMINISTRATION OF CHLORPROMAZINE AND SECOBARBITAL (QUINALBARBITONE) IN SCHIZOPHRENIC PATIENTS*

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IN two previous studies (3 and 4) certain psychological effects of single doses of chlorpromazine and secobarbital (quinalbarbitone) were studied in young normal subjects. It was found that 100 and 200 mg. of chlorpromazine had a greater effect on tests of motor co-ordination than did 100 and 200 mg. of secobarbital, respectively, and that 200 mg. of secobarbital had a greater effect on a test related to intellectual functioning than did 200 mg. of chlorpromazine. The results in our study of the effects of single doses of chlorpromazine differed from the results of studies of the effects of chronic administration of chlorpromazine by Lehmann and Hanrahan (5) and Shaten *et al.* (7). These investigators found that chronic administration of chlorpromazine to schizophrenic patients was followed by improvement or only slight impairment in performance on most of the psychological tests used. The differences between the results we have obtained following administration of single doses of chlorpromazine to normal subjects, and the results of Lehmann and Hanrahan and Shaten *et al.* could be attributed either to differences between the effects of chronic and acute administration of chlorpromazine or to differences in the response between normal subjects and schizophrenic patients.

The purpose of the present study was to shed further light on these drug effects by comparing the response to chronic and acute administration of chlorpromazine and secobarbital in young chronic schizophrenic patients. In addition, the effects of acute administration of these drugs to schizophrenics were compared to the effects in normals observed in the previous study. It was not feasible to study the effects of chronic administration of chlorpromazine to normal subjects.

* This paper was presented in part at the 1958 Meeting of the Federation of American Societies for Experimental Biology.

METHODS

Twelve male schizophrenic patients were selected from the population of subacute service of the hospital. Mean age was 30.1 years (range: 21-40); mean years of hospitalization was 3.75 (range: 1-11.5); mean years of schooling was 12.4 (range: 10-16); and mean I.Q. was 99.1 (range: 84-127). No attempt was made to select any single subtype of schizophrenia; the main criteria for selection were co-operativeness, a minimum of one year of hospitalization, and a maximum age of 40 years. With one exception, the same subjects were used in both the acute and chronic studies.

Five of the subjects were receiving chlorpromazine for therapeutic purposes prior to the start of the experiment, and three additional subjects had received chlorpromazine sometime during their hospitalization. All medications were withdrawn from the subjects at least two weeks prior to the start of the experiment.

Acute Study

One hundred and 200 mg. doses of secobarbital sodium, and 100 and 200 mg. of chlorpromazine hydrochloride were administered on separate occasions to each subject. Each drug at each dose was given twice so that the subjects received drugs on eight separate days. In addition to the drugs, placebos were given on two separate days. A minimum of 48 hours elapsed between drug treatments. Control measurements were taken one day prior to the start of the experiment, and again at its completion. The experimental design was a modified latin square similar to that employed earlier (3).

All drugs, including placebos, were administered orally in identical capsules. Subjects did not eat breakfast on mornings that drugs were administered. The "double-blind" technique was employed throughout. Ninety minutes after the drug was given the following behavioural measures were recorded:

1. *A modified digit symbol test*: This test is similar to that in the Wechsler-Bellevue test (8) of adult intelligence. In order to minimize learning, a different code was used each time the subject received the test. Score was the number of digits correctly completed in a 90-second period.
2. *Pursuit rotor*: A standard Gerbrands machine rotating at 30 r.p.m. was used. The subject was required to maintain contact between a stylus held in the hand and a small electrical contact on the rotating turntable. Subjects were given eight trials of 30 seconds each. The duration of contact was recorded for each trial. Score was the mean of the eight trials.
3. *Tapping speed*: Subjects were required to tap as quickly as possible with a stylus on a metal plate. Five trials of 10 seconds each were recorded. Score was the mean number of taps for the five trials.
4. *Tachistoscopic recognition of numbers*: A modification of the Dodge tachistoscope (manufactured by Gerbrands) was used. Subjects were presented with a series of numbers consisting of from one digit to six digits at exposure speeds from .01 second up to the exposure time necessary for the subject to get three consecutive numbers of six digits correct. Exposure was increased at set intervals of .02 second. The score was the total number of errors.
5. *Reaction time*: Simple visual reaction time was recorded. These results are reported elsewhere (9).

Chronic Study

A minimum of one week after the completion of the acute study, subjects were placed on a two-week regime of chlorpromazine, secobarbital, or a placebo. All subjects received two weeks of each treatment according to a latin square design. The first week on either drug subjects received 100 mg. at 8.00 a.m. and at 8.00 p.m. for a total daily dose of 200 mg. The second week on either drug subjects received 200 mg. at 8.00 a.m. and 8.00 p.m. for a total daily dose of 400 mg. Subjects were tested 90 minutes after the morning medication on the fifth day of each drug week. Breakfast was omitted on testing days. The same tests employed in the acute study were used in the chronic study.

Treatment of Data

An analysis of variance (2) was computed from the data for each test. Since the usual *t*-test does not take into account the number of comparisons that were made, the significance of differences between the effects of placebos and drugs was obtained by the Dunnett *t*-test (1). The test of significance of differences between the effects of various doses of the drugs was computed by means of the Scheffé test (6). These two tests adjust the *P* level according to the number of comparisons made; the greater the number of comparisons, the greater the difference needed for significance.

Tables I and II show the results of the summaries of the analyses of variance

TABLE I

Summaries of Analyses of Variance for All Tests on the Acute Study

Source	df	Tapping Speed		Digit Symbol		Tachistoscopic Threshold		Pursuit Rotor	
		M.Sq.	F	M.Sq.	F	M.Sq.	F	M.Sq.	F
Drugs (D) ..	4	187	6.45*	249	5.93*	194	1.76	104	9.45†
Individuals (I)	11	754	62.80*	1,618	52.20†	1,657	3.17†	91	1.62
D × I ..	44	29	2.42†	42	1.35	110	0.21	11	0.20
Order ..	1	39	3.25	320	10.32†	2,651	5.08*	200	3.57
Error ..	59	12	—	31	—	522	—	56	—
Total ..	119	—	—	—	—	—	—	—	—

* $P < .05$, † $P < .01$.

TABLE II

Summaries of Analyses of Variance for All Tests on the Chronic Study

Source	df	Tapping Speed		Digit Symbol		Tachistoscopic Threshold		Pursuit Rotor	
		M.Sq.	F	M.Sq.	F	M.Sq.	F	M.Sq.	F
Drugs ..	5	106	6.62†	177	6.32†	86	1.41	84.2	10.94†
Individuals ..	10	615	38.44†	972	34.71†	570	9.34†	41.0	5.32†
D × I ..	50	16	—	28	—	61	—	7.7	—
Total ..	65*	—	—	—	—	—	—	—	—

* One subject did not complete the experiment so that computations are based on 11 subjects.

† $P < .01$.

for the acute and chronic study, respectively. It can be seen that on both the acute and chronic studies there was a significant *F* ratio for drugs on all tests except the tachistoscopic recognition of numbers. A significant *F* ratio for individuals was found on all tests in the chronic study, and in all tests but pursuit rotor in the acute study.

Figures 1 to 4 show results on individual tests for acute and chronic administration of both drugs. In each case performance was plotted as a percentage of the mean of the two placebo administrations.

Acute Study

Neither 100 mg. of chlorpromazine nor 100 mg. of secobarbital significantly impaired performance on any of the four tests. Both 200 mg. of chlorpromazine and 200 mg. of secobarbital caused significant impairment in performance on all tests ($P < .01$ on tapping speed and pursuit rotor and $P < .05$ for chlorpromazine on the digit symbol test) except tachistoscopic recognition.

A comparison between each of the drugs at each of the two dose levels indicated that on the tapping speed test 200 mg. of secobarbital produced significantly more impairment than both 100 mg. of secobarbital and 100 mg. of chlorpromazine ($P < .05$). On the digit symbol test 200 mg. of secobarbital caused significantly more impairment than 100 mg. of secobarbital ($P < .05$) and 100 mg. of chlorpromazine ($P < .01$), and 200 mg. of chlorpromazine had significantly more effect than 100 mg. of chlorpromazine ($P < .05$). On the pursuit rotor test 200 mg. of chlorpromazine had significantly more effect than both 100 mg. of chlorpromazine and 100 mg. of secobarbital ($P < .01$). No other comparisons yielded statistically significant differences.

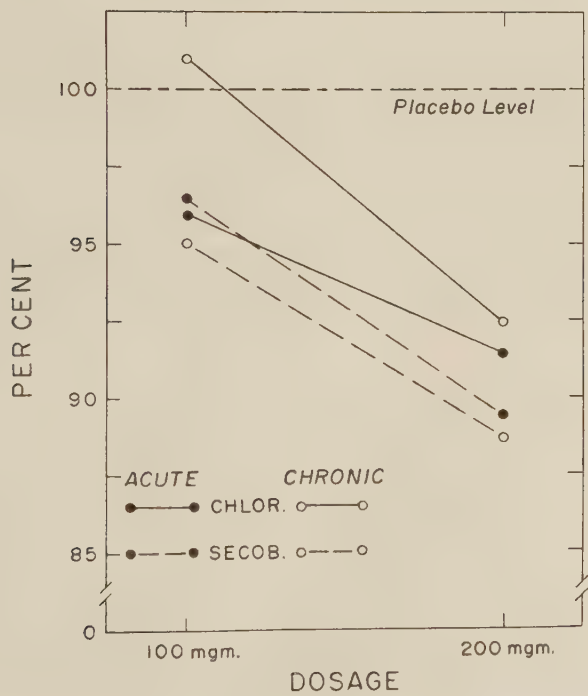


FIG. 1.—Comparison of acute and chronic administration of chlorpromazine and secobarbital plotted as a per cent. of the placebo score on tapping speed test.

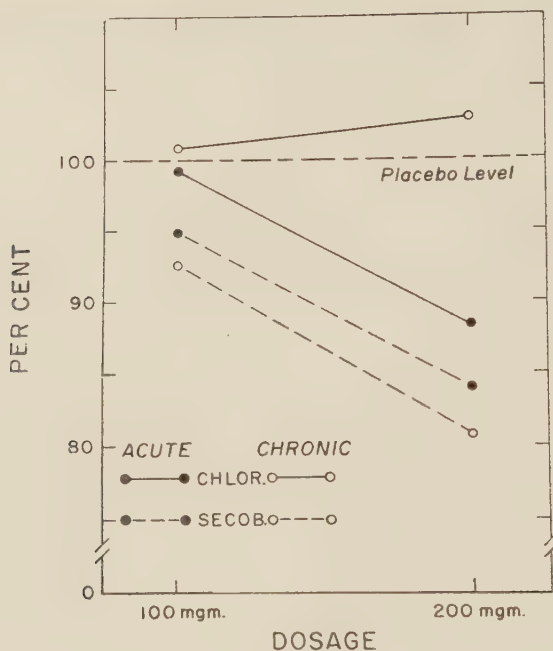


FIG. 2.—Comparison of acute and chronic administration of chlorpromazine and secobarbital plotted as a per cent. of the placebo score on the digit symbol test.

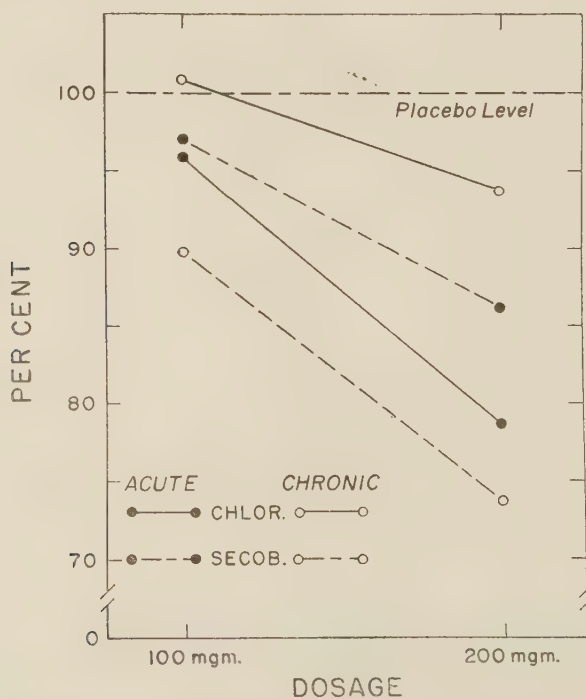


FIG. 3.—Comparison of acute and chronic administration of chlorpromazine and secobarbital plotted as a per cent. of the placebo score on pursuit rotor test.

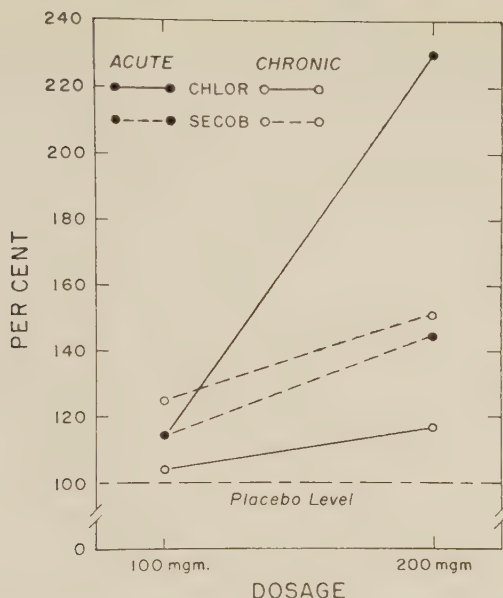


FIG. 4.—Comparison of acute and chronic administration of chlorpromazine and secobarbital plotted as a per cent. of the placebo score on the tachistoscopic perception test (higher score indicates poorer performance).

Chronic Study

The results after chronic administration of the drugs showed that chlorpromazine, which had the most marked effects on performance in the acute study, caused no statistically significant impairment in performance of the tests after chronic administration. Two hundred mg. of secobarbital caused significant impairment in functioning ($P < .01$) on all tests except tachistoscopic recognition. On every test except tachistoscopic recognition, 200 mg. of secobarbital produced more impairment than both 100 and 200 mg. of chlorpromazine ($P < .01$). On the pursuit rotor, the effect of 200 mg. of secobarbital was significantly greater than 100 mg. of secobarbital ($P < .05$).

DISCUSSION

Comparison of the results of this study with the previous study of Kornetsky and Humphries (2) indicates both similarities and differences between schizophrenic patients and normal subjects in their responses to single doses of chlorpromazine and secobarbital. Secobarbital seems to cause approximately as much impairment in functioning in schizophrenics as it does in normals; however, although 200 mg. of chlorpromazine significantly impaired performance of patients on all tests except tachistoscopic recognition of numbers, 100 mg. did not significantly affect performance of the schizophrenic patients on any of the tests. This was not true in the normal subjects. Normal subjects were significantly impaired by 100 mg. of chlorpromazine on tapping speed and on the pursuit rotor. In addition, 200 mg. of chlorpromazine produced less impairment in the schizophrenic patients than in the normal subjects on tachistoscopic

recognition of numbers (Fig. 5) and on the pursuit rotor (Fig. 6). On the tachistoscopic test 200 mg. of chlorpromazine caused no statistically significant impairment in schizophrenics, whereas it produced marked impairment in

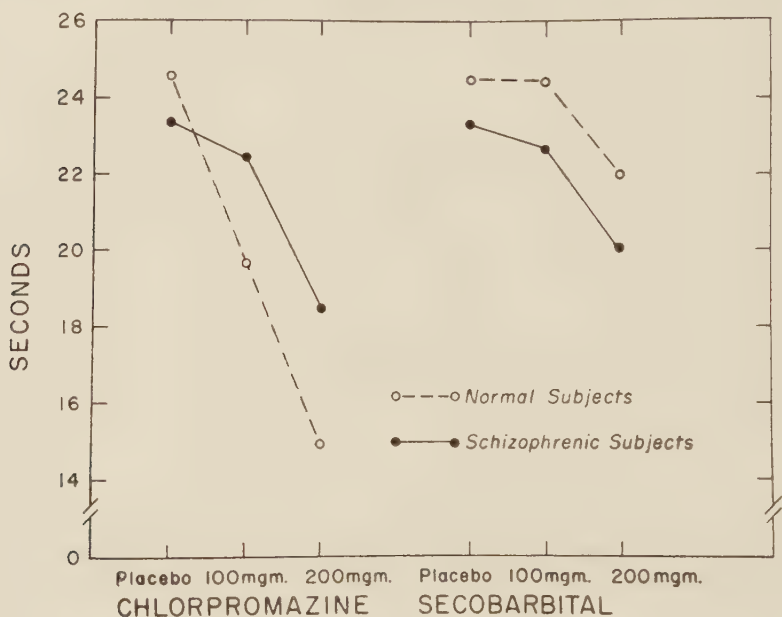


FIG. 5.—The effects of acute administration of chlorpromazine and secobarbital on the performance of schizophrenic and control subjects on the pursuit rotor test.

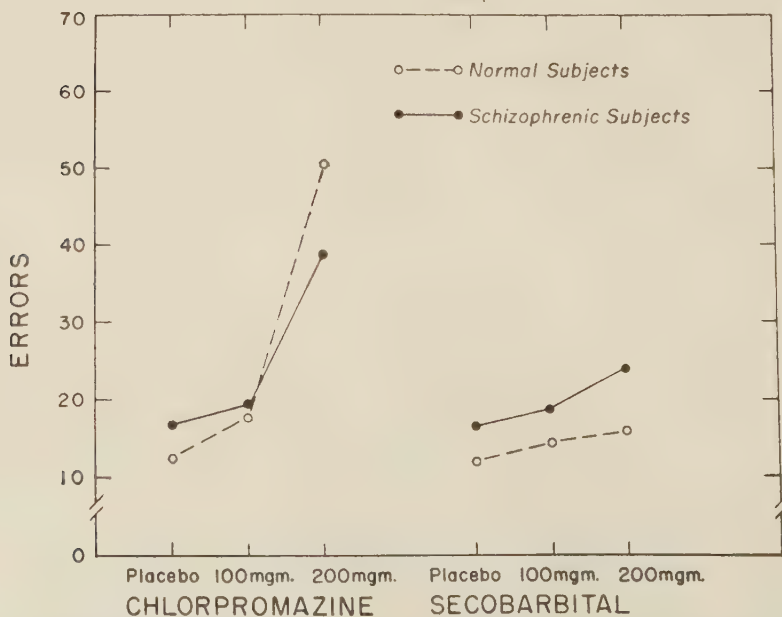


FIG. 6.—The effects of acute administration of chlorpromazine and secobarbital on the performance of schizophrenic and control subjects on the tachistoscopic perception test.

normals. Two hundred mg. of chlorpromazine caused a lesser degree of impairment in schizophrenic than in normal subjects on the pursuit rotor test. On both these tests, the schizophrenic patients performed more poorly than did normal subjects after placebo administration, but better than normals after 200 mg. of chlorpromazine. Figures 7 and 8 compare the performance of patients and

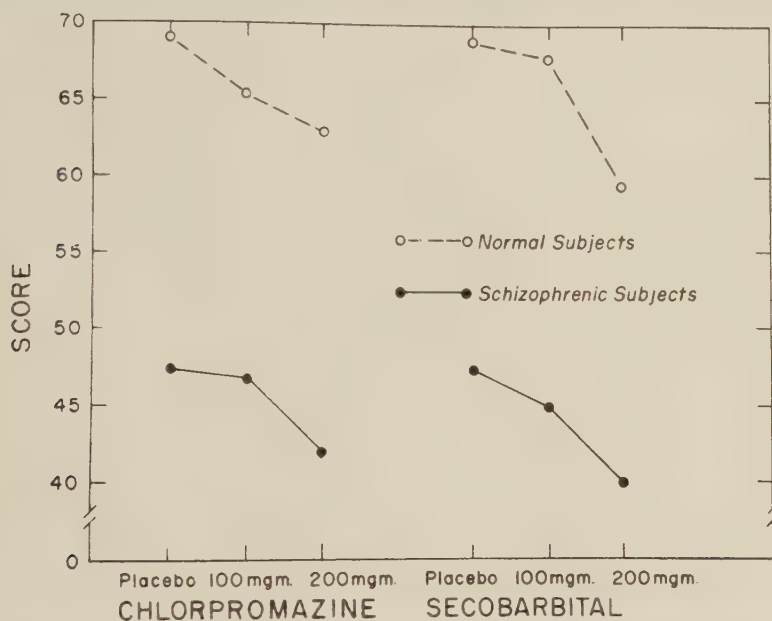


FIG. 7.—The effects of acute administration of chlorpromazine and secobarbital on the performance of schizophrenic and control subjects on the digit symbol test.

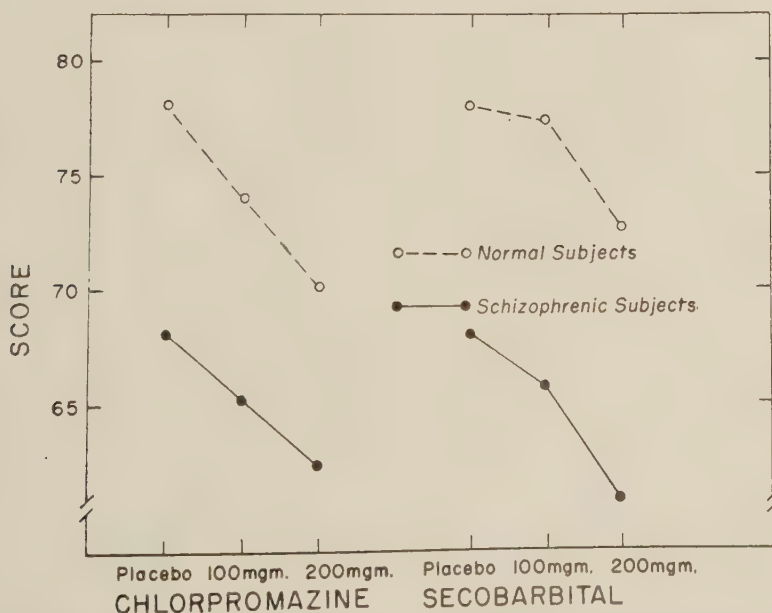


FIG. 8.—The effects of acute administration of chlorpromazine and secobarbital on the performance of schizophrenic and control subjects on the tapping speed test.

normal subjects on the digit symbol test and the tapping speed test. On these tests, the differential effect between normal and schizophrenic subjects was not found.

Daily administration of chlorpromazine to schizophrenic patients produces different effects from acute administration of single doses. At the end of a period of seven days of twice daily administration of 100 mg. of chlorpromazine followed by five days of twice daily administration of 200 mg., tests (given 90 minutes after a 200 mg. dose of chlorpromazine) failed to reveal significant impairment. However, when secobarbital was administered according to the same schedule, a 200 mg. test dose still caused significant impairment in performance, i.e. there was no evidence of tolerance. Single acute doses of 100 mg. of chlorpromazine produced a slight but not statistically significant effect. This small mean effect was no longer apparent after chlorpromazine had been administered for five days at a dose rate of 100 mg. twice daily. This suggests that tolerance to these effects is readily achieved in schizophrenic patients.

The results of this experiment indicate that the differences between the effects of single acute doses of chlorpromazine in normals (Kornetsky and Humphries (3)) and the effects of chronic administration of this drug may be attributed to the differences between chronic and acute administration of the drug. Our failure to observe improvement in performance after chronic administration of chlorpromazine may have been the result of the criteria we employed in the selection of patients. Whereas we used subjects who were co-operative, other investigators who have observed improved test performance following chlorpromazine may have selected subjects who were more disturbed and who were functioning at a level of performance far below their potential prior to the start of drug administration.

SUMMARY AND CONCLUSIONS

Twelve male schizophrenic patients who had been hospitalized for one or more years were tested on four psychological tests after both single doses and chronic (11 days) oral administration of chlorpromazine and secobarbital. The effects of single doses of both drugs were compared to those effects found in normal subjects after single oral doses of the same drugs in a previous experiment.

The following conclusions were drawn from the data:

1. Single doses of 200 mg. of both drugs caused a significant deficit in test performance.
2. After 11 days of chronic administration (seven days of 100 mg. twice daily and four days of 200 mg. twice daily) a 200 mg. test dose of chlorpromazine no longer impaired functioning; a 200 mg. test dose of secobarbital, given after a similar schedule of secobarbital administration, still significantly reduced level of performance.
3. Single doses of 100 and 200 mg. chlorpromazine caused less impairment in schizophrenic patients than in normal subjects.
4. Secobarbital impairs performance of schizophrenic patients and normal subjects to the same extent.

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THE RELATIVE EFFICACY OF "VESPRAL" AND CHLORPROMAZINE IN THE TREATMENT OF A GROUP OF CHRONIC SCHIZOPHRENIC PATIENTS

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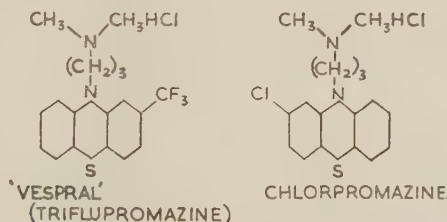
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INTRODUCTION

VESPRAL, or triflupromazine Squibb, is a new phenothiazine derivative. Chemically the compound is 10-(3-dimethylaminopropyl)-2-(trifluoro methyl) phenothiazine hydrochloride. Its structural formula compares with that of chlorpromazine as follows:



Until recently "Vespral" has been available in this country only for clinical trials, but its supposed therapeutic effects have been investigated fairly extensively in the United States and Canada (Goldman; Gallagher and Pfeiffer; Niswander, Lind and Schlesinger; Freed; Azima; Pennington; Saunders; Freyhan; Cahn and Levy).

In a "general summary" made available by the medical division of the Squibb Institute, the reports of clinical trials of Vespral, from a dozen different investigators were summarized as follows: "In general Vespral is as effective as chlorpromazine, in some instances more effective, in controlling active psychotic manifestations. Vespral ameliorates such psychotic symptoms as delusions and hallucinations. It modifies intractable psychotic behaviour patterns, controlling agitation and panic as well as aggressive and hostile behaviour".

A perusal of the side-effects reported in these studies, covering approximately six hundred patients, showed that a Parkinsonism-like syndrome occurred in approximately 12 per cent. of the patients. A hypotensive effect

was observed in patients with elevated blood pressure and postural hypotension was seen occasionally in normotensive persons. References were made to the infrequent occurrence of symptoms such as dizziness, nausea, weakness, drowsiness and epigastric distress. Six patients developed rash, marked photosensitivity developed in two, while three patients attempted suicide while on the drug. No blood dyscrasias or jaundice have been reported to date.

The work on Vespral reported so far appears to be of little scientific value because of the poor experimental designs. Amongst the major limitations of these studies were:

- (i) No controls were used.
- (ii) No statistical treatment of the data was provided.
- (iii) No attempts at objective measurement of the symptoms or behavioural abnormalities under consideration were made.
- (iv) Age, length of hospitalization, degree and duration of illness were not controlled.
- (v) Such heterogeneous groups of patients have been included in certain studies that it has become impossible to correlate the effects of the drug with a specific psychiatric syndrome.

In view of the widespread popularity of chlorpromazine in psychiatric practice today it was considered that any drug which might be its therapeutic superior or equal, and less likely to be associated with the two potentially lethal complications of jaundice and agranulocytosis, was worthy of controlled experimentation.

EXPERIMENTAL DESIGN

The experimental method chosen was that of matched groups. To the groups which would receive one or other of the drugs under comparison was added a third (control) group, which would receive a placebo.

Selection and Description of the Sample

The subjects were sixty-six female chronic patients, all having a diagnosis of schizophrenia. They were chosen from four wards in the annexe of a large mental hospital, these wards being considered representative of the wide variety of symptoms and behaviour patterns to be found in a chronic schizophrenic population. Table I summarizes data on age, and the number of years spent by these patients in hospital. It also summarizes their within-hospital adjustment* scores and their pre-"treatment" ratings on psychotic and withdrawal

* The Within-Hospital Adjustment Inventory (Egan, Walton, Black) was designed to measure the social and occupational adjustment of the patient in hospital, and the degree of nursing supervision that was required. The inventory yields a total within-hospital adjustment score. The inventory was administered to the whole of a mental hospital's chronic population of 1,659 patients. The scale was validated against psychiatric assessment by means of a sample of 200. This sample was representative of the total 1,659 patients in respect of age and sex distribution, diagnosis and record-card adjustment score. Correlations for the validation sample between a set of standard psychiatric criteria and the sections of the adjustment scale separately and together, ranged from .45 to .73. Differences between the mean adjustment scores of the open and closed wards were highly significant. Correlations between different sections of the adjustment scale which had correlated most similarly with the outside psychiatric criteria, gave reliability coefficients from .49 to .73. When corrected according to the Spearman-Brown Formula they yielded correlations of .65 to .84. Those wards most deviant in their mean adjustment scores from the psychiatrically ranked list were seen to include a large proportion of the less reliable of the nursing staff.

scales. The latter two scales are broadly described in "assessment of response" below.

TABLE I

Means, Ranges and Standard Deviations for "Age", "Number of Years Spent in Hospital", "Hospital Adjustment Score", "Rowell" (Psychotic) and "Venables" (Withdrawal) Ratings

Group				Mean	Range	S.D.
				Age		
Vespral	..	..	..	40.00	27-50	5.81
Chlorpromazine	..	..	..	39.59	28-50	7.10
Placebo	..	..	..	39.86	30-50	5.53
				Years in Hospital		
Vespral	..	..	..	13.35	3-28	5.92
Chlorpromazine	..	..	..	12.59	1-24	5.35
Placebo	..	..	..	12.86	3-25	5.49
				Within-hospital Adjustment		
Vespral	..	..	..	27.00	13-41	6.90
Chlorpromazine	..	..	..	26.19	15-43	7.27
Placebo	..	..	..	24.91	11-35	6.53
				Rowell (Psychotic Rating Scale) Scores		
Vespral	..	..	..	51.90	32-67	10.62
Chlorpromazine	..	..	..	50.50	29-67	9.64
Placebo	..	..	..	50.36	28-69	10.46
				Venables (Activity Withdrawal Scale) Scores		
Vespral	..	..	..	25.40	10-47	8.54
Chlorpromazine	..	..	..	24.77	13-40	7.30
Placebo	..	..	..	23.68	10-37	7.54

Matching of Groups

The data as summarized for each group are presented in Table I. The five variables contained therein (age, length of hospitalization, etc.) were employed as a basis for dividing the sixty-six subjects into three matched groups of twenty-two persons each. Table II contains a detailed analysis of the final selection of the patients in the three experimental groups. It can be seen that

TABLE II

Numerical Incidence in Each Group

Criterion				Vespral	Chlorpromazine	Placebo
Age:						
30 and under	..	..	..	1	2	1
31-35	..	..	..	5	6	4
36-40	..	..	..	6	3	6
41-45	..	..	..	6	4	6
46-50	..	..	..	4	7	5
				—	—	—
				22	22	22
				—	—	—

Table II continued

Hospitalization:

5 years and under	..	..	1	3	2
6-10	..	..	7	3	4
11-15	..	..	5	9	10
16-20	..	..	7	5	4
Over 20	..	..	2	2	2
			22	22	22

Hospital Adjustment Score:

9-15	..	..	1	1	2
16-21	..	..	5	6	6
22-29	..	..	7	8	9
30-37	..	..	7	6	5
38-45	..	..	2	1	0
			22	22	22

Rowell Score:

28-36	..	..	3	2	3
37-44	..	..	2	4	3
45-52	..	..	6	6	5
53-60	..	..	6	6	6
61-69	..	..	5	4	5
			22	22	22

Venables Score:

47-40	..	..	1	1	0
39-33	..	..	4	3	4
32-26	..	..	5	5	4
25-18	..	..	8	9	9
17-10	..	..	4	4	5
			22	22	22

Ward:

2	..	..	4	3	4
8	..	..	6	7	6
10	..	..	7	6	7
14	..	..	5	6	5
			22	22	22

not only has each group been matched on means and standard deviations for each variable as given in Table I but each group was adequately represented at all or any level of the five variables and adequately represented in each of the four wards.

Such close matching between the three groups was achieved by matching a representative of each group simultaneously on all five variables. This resulted in twenty-two matched trios being formed and to obviate an inter-ward variable effecting changes beyond those attributable to differences in drug action, with

a few exceptions all members of each trio were from the same ward. One example of this matching will suffice to illustrate this approach:

Trio No. 5		Ward	Age	Hospitali- zation	Adjust- ment Score	Rowell	Venables
Vespral	..	8	34	13	27	55	23
Chlorpromazine		8	36	16	29	56	21
Placebo	..	8	37	14	33	61	22

No statistically significant differences on any of the matched variables were found to exist between the groups before the experiment began.

Assessment of Response

It is generally accepted that psychotic behaviour is difficult to measure, and yet an objective quantitative measure is necessary for finer statistical evaluation. A search was therefore made of the literature in an attempt to find an instrument by which changes in psychotic behaviour could be measured and at the same time have sufficient reliability and validity. Two scales were chosen as being closest to the ideals required. These were the Rowell (1951) and Venables (1957).

The Rowell graphic rating scale is a means by which the behaviour of psychotic patients in a ward environment can be recorded. It consists of twenty "behaviour" items rated on a five-point continuum. The possible scores range from twenty (least deviant) to one hundred (most deviant). The items cover, for example, such aspects of behaviour as pre-occupation, motor activity, personal appearance, attitude towards and consideration for others, co-operation and sociability. It also contains items for measuring mood, affect, thought process, projective trends, etc.

The Venables rating scale measures "activity-withdrawal", using ten items rated on a five-point continuum. It can yield scores ranging from ten (most withdrawn) to fifty (most active). As the scoring of these scales required close and almost continuous contact with and observation of the patient, such requirements in the raters were only available among members of the nursing staff. It was decided that the ratings of the patients in a particular ward would be done throughout by the same nurse as this would help to standardize the rating of a particular patient. For three of the wards the sister in charge was chosen as a rater for the subjects in her ward. In the fourth ward the deputy sister was chosen because of the anticipated absence of the ward sister before the completion of the experiment.

Prior to the initial ratings—on which the groups were matched—both scales were explained to and discussed with these four raters. The boundaries and degrees of each item were outlined and any anticipated ambiguities were clarified. Ratings were done weekly, the rater being instructed to select the point on each item of the scale most applicable throughout the preceding week. The doctor was present at each assessment in order to clarify any doubtful points in terminology or boundaries of an item point, but his presence in no way affected the decisions of the rater. Raters were not asked to make comparison with their previous assessments and they did not have records of or access to previous assessments.

Statistical Methodology

The usual parametric test for assessing the significance of the difference score between two related samples is the *t* test. Certain conditions must be satisfied, however, before any probability statement based on the *t* test can be accepted with confidence. The major assumptions are, firstly, that the difference scores are normally distributed in the population from which the subjects are drawn, and secondly are measured on an interval scale.

It is doubtful whether the scores of the two measuring instruments used in this study (Rowell and Venables) are sufficiently exact to be treated numerically. It is not possible to say, for example, that a schizophrenic whose Rowell score is 70 is twice as disturbed as one whose score is 35. Nor is it possible to say with confidence that the difference score between 70 and 50 is exactly twice as large as the difference score between 50 and 40. It is possible to say, however, that the difference score between 70 and 50 is *greater* than the difference score between 50 and 40, so that the difference scores can be ranked in order of absolute size.

A further doubt as to the applicability here of the *t* test arises because of the lack of available evidence to suggest that the difference scores are normally and independently distributed in the schizophrenic population from which the present sixty-six cases were drawn.

The most appropriate statistical test for the present research design appears to be a two-sample non-parametric test. This type of statistic is used when the researcher wishes to determine whether one treatment exerts a more beneficial effect than another and when each individual of a pair of subjects to receive different preparations is as much alike as possible to the other individual in the pair with regard to any extraneous variables which might affect the interpretation of the results. No assumptions are made as to distribution.

The Wilcoxon Matched-Pairs Signed-Ranks Test (Wilcoxon, 1949 and Siegel, 1956) is an example of this type of statistic and appears appropriate for the analysis of the present data. This utilizes information both from the direction of difference scores within matched pairs and also considers the magnitude of these differences.

The rationale of the test is that if A and B are equally effective, that is if the null hypothesis (H_0) be true, then some of the larger difference scores (d_1) for any matched pair would favour treatment A and some favour treatment B. Some of the higher rankings would come from positive d_1 's while others from negative d_1 's. The sum of the positive d_1 's rankings and the sum of the negative d_1 's rankings would, according to H_0 , be about equal. If the sum of the positive d_1 ranks were very different from the sum of the negative d_1 ranks then one could infer that treatment A differed from treatment B, and that H_0 could be rejected.

Administration of the Preparations

Allocation of a preparation to a group was purely arbitrary. All preparations were given orally and were given simultaneously for eight weeks. The scheme of dosage was as follows: for those patients on active preparations, week 1, 25 mg. t.d.s., weeks 2 and 3, 50 mg. t.d.s., weeks 4 and 5, 75 mg. t.d.s., weeks 6, 7 and 8, 100 mg. t.d.s. As this involved periodic changes in the number and/or size of the tablets, the number and/or size of the tablets given to the placebo group was altered simultaneously. Neither the doctor nor the ward staff were aware of the nature of the preparation being given to a particular

patient. A separate bottle was assigned to each patient and in this supplies of the specific preparation were issued at weekly intervals by one of the authors (D.A.B.) in accordance with the pre-arranged dosage scheme outlined above, except where individual modifications in dosage appeared to be indicated. The factors leading to such divergence from the general scheme of dosage are discussed under "Results".

Special Investigations

(i) Sedation threshold: The frequent appearance of contradictory findings from different investigations concerning the efficacy of a particular drug is only too well known. Many potential reasons for this lack of agreement can be proffered. Many notional and empirical studies can be criticized for their experimental, statistical, and methodological errors (Eysenck, 1957). Clearly illustrative of this are the serious differences to be expected in the efficacy of a drug according to whether the investigation includes controls or not. Foulds (1958) for example, found in a survey of Anglo-American drug investigations, covering the years 1951-56, that success was claimed in 83 per cent. of uncontrolled studies as against 25 per cent. in the controlled studies. Even apparently well-controlled studies are limited in the extent to which they can predict the usefulness of a preparation in a particular case. This is because the drug was manufactured on either notional or empirical considerations rather than on a rational basis.

An encouraging and possibly fruitful line of approach towards the discovery of a rational method of drug administration in psychiatry, is the recent attempt to integrate drug action and personality theory (Eysenck, 1957). One aspect of this has been the work of Shagass (1954, 1956) on the sedation threshold. His work suggests that the sedation threshold is a variable which might well be taken into account in comparative studies of this nature. To take an example, let it be supposed that the relative efficacy of two preparations is under investigation and two matched groups are employed for a fixed experimental period. Then, if one group consists of subjects having significantly higher sedation thresholds than the other, preparations of equal potency may show unequal therapeutic effects within such a fixed period.

Ideally we would have liked to calculate the sedation threshold of all our subjects in order to control this variable. But, as can be expected, many of the patients were, from a behavioural point of view, totally unsuitable for the procedure involved, and only twenty-eight patients were considered to have the necessary degree of co-operation required. Of these twenty-eight patients only sixteen yielded a reliable result. In the group matchings it was endeavoured to place these patients in one or other of the two groups receiving an active preparation. In only one case was this not achieved, the distribution being: Vespral group—8, Chlorpromazine group—7, Placebo group—1.

(ii) White cell counts: Serial white cell counts were decided against for the following reasons:

- (a) To reduce to a minimum the number of factors which might produce improvement irrespective of any possible drug action.
- (b) To avoid an inter-group variable the procedure of withdrawing blood would have had to be followed in all groups. Where the Placebo group was concerned, this procedure was not considered justifiable.

Instead it was decided to do one white cell count on the patients in the Vespral and Chlorpromazine groups after they had received their final assessment.

(iii) Blood pressure estimations: These were done weekly by one of the authors (G.P.W.). As he was throughout unaware as to which patients were on an active preparation, and which were on a placebo, and also to avoid introducing an intergroup variable, these estimations were done on all sixty-six patients, with the exception of one, who was throughout too resistive, but on whom nevertheless weekly attempts at estimation were made.

Apart from the weekly blood pressure estimations, and the administration of the preparations, no other new factors were introduced into the patients' routine throughout the course of the experiment.

RESULTS

Assessment of Therapeutic Effects

In the present experiment twenty-two matched trios served as subjects. H_0 stated that neither Vespral nor chlorpromazine exerted a therapeutic effect greater than a placebo*. The alternative hypothesis (H_1) was: both Vespral and chlorpromazine exert a therapeutic effect greater than a placebo and moreover Vespral exerts a greater therapeutic effect than chlorpromazine†. Table G (Siegel) provides the critical values of T on the Wilcoxon Matched-Pairs Signed Ranks Test for the sampling distribution (the smaller sum of like signed ranks) where $N < 25$. The sum of the ranks with the less frequent sign for the six possible comparisons, based on the mean score throughout the eight weeks trial period, and their levels of significance, are as follows:

Rowell "Psychoticism" Rating Scale

<i>Comparison</i>	<i>T</i>	<i>Level of Significance</i>
Vespral and chlorpromazine	105.0	Not significant
Vespral and placebo	Ranks equal	Not significant
Chlorpromazine and placebo	76.0	Not significant

Venables "Activity-Withdrawal" Rating Scale

<i>Comparison</i>	<i>T</i>	<i>Level of Significance</i>
Vespral and chlorpromazine	98.5	Not significant
Vespral and placebo	101.0	Not significant
Chlorpromazine and placebo	Ranks equal	Not significant

The results thus fail to reject the null hypothesis. On the variables measured and in the conditions described neither Vespral nor chlorpromazine exerted a significantly greater effect than placebo, nor did Vespral exert a significantly greater effect than chlorpromazine, although all three groups showed considerable improvement on both Rowell and Venables scales during the course of the experiment (see Table III).

The trial therefore fails to show that either Vespral or chlorpromazine exerts an influence beyond that attributable to a "placebo effect".

Precisely what gives rise to such an effect is uncertain, though it would probably include such factors as change of routine and increased attention (thrice daily receipt of a tablet, weekly blood pressure estimation, etc.).

* In terms of the Wilcoxon test, the sum of the positive ranks (d_1 's)=the sum of the negative ranks (d_2 's).

† I.e. the sum of the positive ranks/the sum of the negative ranks.

TABLE III

Group	Rowell Rating Scale Group mean score			Venables Rating Scale Group mean score		
	Before Trial	8th Week	Difference	Before Trial	8th Week	Difference
Vespral ..	51.9	36.9	15.0	25.4	27.4	2.0
Chlorpromazine ..	50.5	33.4	17.1	24.8	28.4	3.6
Placebo ..	51.7	39.1	12.6	24.6	27.0	2.4

Clinical Impressions

At the conclusion of the trial period, when every patient's last rating scale assessment had been completed, each sister from the four wards involved was also asked to say how she felt the various patients in her charge had progressed during the trial. As well as remarking on any features of special note (e.g. patient more active, more co-operative, etc.) each sister was asked to say, which of the following four categories most nearly described the patient's progress:

"Much improved", "Slightly improved", "Unchanged", or "Worse".

Table IV shows the number of patients out of the sixty-six, falling into each category.

TABLE IV

	Much Improved	Slightly Improved	Unchanged	Worse
Vespral	10	9	2	1
Chlorpromazine ..	11	9	2	0
Placebo	5	9	8	0

The four sisters contributing to this assessment were still, at that time, unaware of the particular preparation each patient had been receiving and were also under the impression that all three preparations were active ones.

Special Investigation—Sedation Threshold

Sedation Threshold was employed as a basis for dividing sixteen subjects (on whom reliable sedation thresholds had been recorded) into two matched groups of six patients each. One group received chlorpromazine whilst the other received Vespral. The mean Rowell and Venables scores for the two groups over the eight weeks experimental period were then compared statistically by means of the Wilcoxon Matched-Pairs Test. No significant differences emerged between the effects produced by chlorpromazine and Vespral when sedation threshold was controlled. Absence of significant differences on the Rowell and Venables scales between the two groups cannot therefore be attributed to differences in sedation threshold.

Side-Effects

(i) Drowsiness: This appeared in four of the patients receiving Vespral, two of those receiving chlorpromazine, but none of those receiving a placebo. In two of the Vespral subjects drowsiness occurred in the third week while on 50 mg. t.d.s., and disappeared on reduction to 25 mg. t.d.s. (this reduction was made because of the appearance of marked hypotension and not because of the drowsiness). In the other two Vespral subjects, and in those two receiving chlorpromazine, drowsiness only appeared in the latter half of the trial period and persisted to the end of the trial in only two patients, one on Vespral and one on chlorpromazine.

(ii) Pallor: This was reported in four cases, all in receipt of Vespral, and occurred in the latter half of the trial.

(iii) Parkinsonian features: Five patients, all receiving Vespral, showed some degree of Parkinsonism. In three of these this was limited to the development of "mask-like" facies, whilst in the other two rigidity developed. In no case, however, was withdrawal of the drug necessitated.

(iv) Rash: This occurred in one patient in the chlorpromazine group, in the fifth week of the trial, and subsided on withdrawal of the drug.

(v) Oedema: Facial oedema developed in one patient of the Vespral group, in the sixth week of the trial, and subsided on withdrawal of the drug.

(vi) Hypotension: Falls in blood pressure, both systolic and diastolic, occurred in all three groups. The number of patients in each group showing such falls was:

							<i>Systolic</i>	<i>Diastolic</i>
Vespral	..	..	..	..	..	..	19	12
Chlorpromazine	..	..	..	..	..	..	9	10
Placebo	..	..	..	..	..	..	10	10

Some falls were so marked as to call for modification of the dosage scheme in the subjects concerned. Table V shows the number of patients receiving the various dosages in force at the conclusion of the trial.

TABLE V
No. of Patients on Each Dosage at End of Trial

Group		100 mg., t.d.s.	75 mg., t.d.s.	50 mg., t.d.s.	25 mg., t.d.s.
Vespral		9	3	3*	7
Chlorpromazine		13	3†	3	1
Placebo		18	2	1	1

* Including patient withdrawn sixth week.

† Including patient withdrawn fifth week.

The statistical significance of the differences in change of blood pressure between the groups was calculated, taking into account every available weekly blood pressure reading of each patient throughout the trial period. Only the change in systolic pressure of the Vespral group by comparison with the change in systolic pressure of the placebo group proved to be significant ($T=48.5$, $P<0.02$; Wilcoxon Matched-Pairs, signed Ranks Test). No other differences in systolic changes proved to be significant, nor was any group difference in change of diastolic pressure found to be significant.

These findings are in accordance with the reduction in dosage necessitated in many of the Vespral group and shown in Table V. Vespral, in the dosages employed, is thus shown to exert a greater hypotensive tendency than chlorpromazine, which exerts no significantly different effect than a placebo.

No other side-effects were encountered and the white cell counts of those subjects on an active preparation were within the normal range at the conclusion of the experiment.

SUMMARY AND CONCLUSIONS

An experiment was designed to test the relative efficacy of "Vespral", chlorpromazine and a placebo in the treatment of a group of female chronic schizophrenic patients.

The sixty-six patients who took part in the trial were divided into three closely matched groups.

Each group received respectively Vespral, chlorpromazine or a placebo in increasing dosages over a period of eight weeks. Response to treatment was assessed by the Rowell and the Venables rating scales, purporting to measure respectively psychoticism and degree of withdrawal.

The following conclusions can be drawn, subject to any limitations imposed by the length of the experiment and the dosages employed:

1. Both Vespral and chlorpromazine seem to be of limited value in the treatment of the chronic schizophrenic patient. Neither preparation exerted a greater therapeutic effect than a placebo. Little consistency in the direction of response to treatment was recorded. The control trial and the brief clinical evaluation of therapeutic response by the senior nursing staff concerned showed how difficult it would be to predict therapeutic outcome in response to either Vespral or chlorpromazine. Just as many subjects improved as failed to show significant change.

2. As a perusal of the rating scale score changes during the eight weeks experimental period suggested that Vespral exerted the greater part of its total effect earlier than both chlorpromazine and placebo, it is suggested that such an observation might form the basis of a further experiment of appropriate design.

3. The marked hypotensive effect exerted by Vespral as compared with chlorpromazine suggests that the exhibition of Vespral should be accompanied by frequent blood pressure estimations.

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AN INVESTIGATION INTO THE EFFECTS OF AZACYCLONAL ON THE HALLUCINATIONS OF CHRONIC SCHIZOPHRENIC PATIENTS

By

M. A. HARGREAVES

FRENQUEL, α -(4-piperidyl) benzhydrol hydrochloride, generically known as Azacyclonal hydrochloride was introduced commercially into this country in the early part of 1957 although it had already been available in the United States of America for several months previously. Sargent (1956) obtained a supply of Azacyclonal from America and in his paper on Chemical Tranquillizers he refers to Azacyclonal as "an interesting drug". Azacyclonal was thought to be the first drug to possess a specific anti-hallucinatory effect.

Fabing (1955) was the first to acclaim the unusual properties of Azacyclonal when he found that it relieved the confusion and hallucinations of a normal healthy student who had previously been given a dose of lysergic acid diethylamide (LSD-25). The actions of LSD-25 other than hallucinogenic were not affected by the Azacyclonal. These observations prompted a number of further investigations and in 1955 and 1956, several reports of the actions and properties of Azacyclonal appeared in American medical literature. Brown *et al.* (1955) carried out pharmacological tests with Azacyclonal and found that it was mildly depressant in low doses and stimulant in large doses. They also found it had little or no effect on the blood pressure and electrocardiograph in the anaesthetized dog although both the rate and depth of respiration increased. Bettag *et al.* (1955) compared the effects of Azacyclonal, reserpine and chlorpromazine on the cerebral electrical activity of rabbits. Azacyclonal had no effect on the electrical activity of a "normal" brain but it corrected the abnormalities produced by LSD-25 or mescaline.

On the clinical side Fabing and Hawkins (1955) reported the results after a year's experience with the drug. They treated 115 patients altogether and found the responses encouraging but variable from patient to patient. Rinaldi *et al.* (1955) treated 39 chronic patients and found significant improvement in a large proportion of them.

Proctor and Odland (1956) treated three different groups of patients. One group consisted of acute patients, in whom the response was good. The second group were chronic patients and here the results were less satisfactory. The third group all had toxic psychoses, mainly brought about by alcohol, and the improvement which followed administration of Azacyclonal was in some cases dramatic. Ferguson (1956) tried Azacyclonal in 264 chronic female patients and noticed changes in behaviour in a number of them. The results obtained by different workers who have used Azacyclonal and the dosages recommended vary considerably and the only observation which is common to all reports is the complete absence of side-effects which accompanies Azacyclonal therapy.

In view of these findings it was decided to determine the effect of Azacyclonal on the hallucinations of a number of chronic patients.

CLINICAL MATERIAL

The patients chosen for the trial were all women selected from three chronic wards. The duration of their stay in hospital varied from eight to thirty-nine years, averaging 18.33 years. Fifty patients were selected but during the trial four of them, for various reasons, had to be excluded. All were markedly disturbed and all suffered particularly from auditory hallucinations which had not been affected by previous therapy.

METHODS USED

The patients were divided into two groups (A and B) and matched for age (Table I) and severity of symptoms. The method of arriving at the severity rating is explained in Table II.

TABLE I

Group A				Group B			
Severity	Age	Severity	Age	Severity	Age	Severity	Age
9	74	7	56	11	71	9	57
7	73	11	51	9	72	11	52
8	69	11	58	7	66	10	51
10	69	10	57	10	68	7	58
10	68	9	49	9	67	11	47
9	62	7	43	9	64	7	44
8	59	5	44	8	63	6	47
6	61	8	46	5	60	8	47
7	63	8	37	7	64	6	33
8	65	8	56	8	62	10	53
6	52	7	60	4	59	7	64
7	56	9	49	8	59	10	44
		9	63			8	64
Mean 8.16 Age 57.6				Mean 8.2 Age 57.44			

The duration of the trial was three months. During the first half of the trial Group A received placebo tablets and Group B those containing the active preparation. The pattern of administration was then reversed, Group A receiving the active preparation and Group B control tablets. After the first six weeks there was a four-day interval when both Azacyclonal and placebo therapy were withdrawn. The second half was then run exactly as the first but with the administration of placebo and active material reversed. The double blind technique was used. Throughout the trial only the pharmacist knew which patients were receiving the active preparation and which were receiving placebo. The key was not revealed until the termination of the trial.

Laboratory investigations included haemoglobin determinations, white cell counts and blood sedimentation rates. These were carried out weekly with each patient for a total of thirteen weeks in each case. No significant change was noticed in any patient and it was therefore considered unnecessary to investigate the blood picture further. No autonomic side-effects or any other toxic effects were observed clinically. The severity rating of each patient was calculated (as in Table II), before the trial commenced and at the end of the first and last halves. In addition day to day assessments were made by the observers and any change carefully noted.

TABLE II

	Sister	Sister	Doctor	Doctor	
	Frequency of Hallucination	Response to Hallucination	Reality Basis of Hallucination	Degree of Insight	Severity Rating
I					
II					
III					

Frequency:

Hallucinations noticed once or twice during day	= 1
Hallucinations noticed several times during day	= 2
Hallucinations noticed very frequently or continuously	= 3

Response to Hallucinations:

Attending	= 1
Answering and responding	= 2
Answering and responding violently	= 3

Severity Rating: This was taken as the sum of the points allotted to each patient in the four columns of the table.

Reality Basis:

Mainly rational	= 1
Mainly irrational	= 2
Bizarre	= 3

Insight:

Some	= 1
Little	= 2
None	= 3

Mean:

Group A	— 8.16
Group B	— 8.2

DOSAGE

The initial daily dose of Azacyclonal was 60 mg. and every fourth day this was increased by a further 60 mg. up to a maximum of 420 mg. daily given in three equally divided doses. To avoid unnecessary complications only the oral form of Azacyclonal was employed. Any other treatment the patients were receiving was continued during the course of the trial.

RESULTS

With doses less than 300 mg. daily little or no effect was apparent in any patient. When a dosage of 300 mg. daily was reached, it was noted that a percentage of the patients improved from a social point of view, although the hallucinations were not specifically influenced. As the original purpose of the trial was to assess the anti-hallucinatory effect of Azacyclonal no provision had been made on the assessment tables for any change in social behaviour to be noted. It is therefore not possible to present a statistical analysis of these changes. Both active and control material produced this effect.

These improvements included:

1. A better social contact and an ability to converse with other patients.
2. Sufficient responsibility to be allowed ground parole.

3. Better attention to personal hygiene with a subsequent reduction in nursing supervision.
4. Ability to do light domestic work in the ward.

Observations have indicated that the above improvements were slightly more apparent on patients receiving the placebo preparation as opposed to the active drug. Of the 46 patients, 7 became noisier and appeared to deteriorate whilst on active material.

Since hallucinations were not affected in any way, statistical analysis was not considered worth while.

DISCUSSION

Originally this controlled experiment was designed only to determine the effects of Azacyclonal on auditory hallucinations in chronic patients. In each case it is of significance that the hallucinations had been present for many years and had not responded to previous therapy. As some confusion exists about the optimum dosage of this drug an attempt was made to clarify the position. Four days were allowed for assessment at each dose level. The literature indicated that 300 mg. daily was a reasonable dose, but as this was by no means proven, it was decided to increase the dose to 420 mg. Below 300 mg. daily no clinical effect was observed.

It is disappointing that the results obtained indicate that Azacyclonal is of little or no value in chronic hallucinated patients.

It is noteworthy that three patients each suffering from hallucinations during the acute initial stages of mental illness were treated with Azacyclonal on a dosage of 240–300 mg. daily. The impression gained was that they had improved considerably. This would support the findings of Proctor and Odland (1956).

The social improvement mentioned may well have been due to the fact that more attention was being paid to the patients, and that they were receiving a new treatment.

SUMMARY

An attempt has been made to evaluate Azacyclonal in fifty chronically hallucinated female patients. The dosages used varied from 60 to 420 mg. a day in order to find the dose level producing the maximum therapeutic effect.

Observations in this trial indicate that the average optimum dose is in the region of 300 mg. daily. No side-effects whatsoever were recorded even when 420 mg. daily were given despite thorough laboratory examinations. On the basis of the effect on chronic hallucinations it must be admitted that the results of the trial are disappointing, and it would appear that Azacyclonal is of no value in the treatment of this kind of patient. It is significant that a small number of patients with acute hallucinations have responded dramatically to treatment with Azacyclonal.

If enough clinical material could be accumulated a trial of this drug on acutely hallucinated patients might produce very interesting results.

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PROCHLORPERAZINE (STEMETIL) IN MENTAL DEFICIENCY

By

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INTRODUCTION

THE use of tranquillizers in mental deficiency has shown a divergency of effects. Noce, Williams and Rapaport (1) claimed that reserpine produced some improvement in all their thirteen adult hyperactive low-grades, and Rettig (2) reported that chlorpromazine produced decreased hyperactivity in twenty-three of his twenty-seven adult defectives of all grades. Blair and Herold (3), using ten "matched-pairs" of hyperactive defective children, found behaviour improvements in nine of them on chlorpromazine, with an average I.Q. gain of 10.4. Using six different tranquillizers on twenty-five adult defectives of all grades, Rudy, Himwich and Rinaldi (4), found that all gave some benefit, with chlorpromazine the most effective. Fischer (5) showed that twenty out of his twenty-two low-grade adult defectives (many with psychotic overlay) were improved in behaviour on reserpine. Using sixteen "matched-pairs" of E.S.N. children, Segal and Tansley (6) claimed that hydroxyzine produced improved scholastic performance in fourteen of them, and Bergin and Bergin (7) stated that the epileptics in particular in their thirty-four low-grades were improved by promazine hydrochloride.

On the other hand Gillie (8) found that mepazine had little effect on her thirty low-grades, and Craft (9) obtained no effect on his twenty-three low-grade adults with hydroxyzine. The same author (10) found no improvement in his sixteen hyperactive adults with chlorpromazine, and four developed stupor, collapse and severe excitation.

In view of this, the purpose of the present trial was to try to obtain some indication of the effects of a tranquillizer, prochlorperazine ("Stemetil") on the different types of mental defectives, as well as the overall response. Stemetil is a phenothiazine derivative, incorporating a piperazine ring in the side chain, and has been used for its anti-emetic action in migraine, Ménière's syndrome, etc. In larger doses it is claimed to reduce over-activity, aggression and agitation with less central depression than chlorpromazine, and its main side-effects are disturbances of an extra-pyramidal type. Milne and Berliner (11) showed appreciable behaviour improvement in all their fifty male schizophrenics on 75 mg. of Stemetil daily, but noted slight drowsiness as an occasional accompaniment, with five showing rigidity and tremors.

PROCEDURE AND DOSAGE

Out of approximately 900 patients of all ages and grades, 88 were finally picked as presenting behaviour disturbances of such a degree as to constitute nursing problems. These comprised 21 children between 6 and 14 (average age, 10 $\frac{3}{4}$ years) and 67 adults between 15 and 66 (average age 36 $\frac{1}{2}$ years). Of the

children, 4 were idiots, 14 imbeciles and 3 feeble-minded, their I.Q.s ranging up to 70 per cent. Fourteen attended the hospital school. In the total were 28 males and 60 females, and the adults comprised 42 low-grade (idiots and imbeciles) and 25 high-grade (feeble-minded).

These were all personally assessed clinically along with nursing gradings under the headings of type of behaviour disturbance, degree and type of hyperactivity, work capabilities, recreation, socialization, sleep and personal pride. Each group (children, high-grade and low-grade adults) was further subdivided into epileptics, psychotic overlay—schizophrenics, psychotic overlay—affectives, and simple hyperactives.

Twenty-six had previously been on tranquillizers, with little or no effect, and a further 17 on sedatives. Apart from two who remained on nightly sedation, all other drugs were stopped, with the exception that the epileptics remained on anti-convulsants.

The children weighed between 3 and $6\frac{3}{4}$ stones (average 5 stones) and were given 5 mg. Stemetil t.d.s. for two months. The adults varied between $5\frac{1}{2}$ stones and $18\frac{1}{2}$ stones (average 9 stones), and all started on 12.5 mg. of Stemetil t.d.s., rising to 25 mg. t.d.s. after two weeks for the remainder of the two months' course. Subsequently, 13 adults had to be reduced to between 12.5 mg. and 50 mg. daily, but this had no relation to their weight, the average for this group being $8\frac{1}{2}$ stones. It is significant, however, that this group contained 77 per cent. low-grades in contrast to the 37 per cent. low-grades in the total adults.

Individual mental progress was assessed at two weeks, four weeks, and eight weeks, and urine examinations and blood counts performed during the course showed no abnormalities in any patient. There were no skin rashes or cases of jaundice, but five showed slight drowsiness that did not merit an alteration in dose. Twenty-three per cent. showed other complications, however, which are discussed below. The final mental assessments were made under the headings of "Much improvement", "Some improvement", "No change", and "Worse".

RESULTS

It will be seen from Table I that 65 per cent. of the total cases showed some appreciable response, and that this figure remains approximately the same in the sub-divisions of children, low-grade and high-grade adults.

TABLE I

	Worse	No Change	Some Improvement	Much Improvement
	(Per cent.)	(Per cent.)	(Per cent.)	(Per cent.)
Total cases (88)	9	26	30	35 (65% responded)
Children (21)	4	29	38	29 (67% responded)
Low grade (adults) (42)	10	24	33	33 (66% responded)
High grade (adults) (25)	12	28	16	44 (60% responded)

Table II showed the effects in the different diagnoses, where it is apparent that the schizophrenic group showed the poorest response, and the affectives and simple hyperactives the best, with the epileptics between the two.

TABLE II

	Worse	No Change	Some Improve- ment	Much Improve- ment
	(Per cent.)	(Per cent.)	(Per cent.)	(Per cent.)
Total epileptics (28)	11	28	36	25 (61% responded)
Total psychotic-schizophrenics (12)	0	67	25	8 (33% responded)
Total affectives (16)	12	12	25	50 (75% responded)
Total simple hyperactives (32) ..	9	16	28	47 (75% responded)

When the grades of oligophrenia were further broken down into the psychiatric groups, as in Table III, the schizophrenic group showed the poorest response in all cases, but only in the low-grade adults did those with additional epilepsy show a good response. The simple hyperactives responded fairly well in all grades, as did the affective children and low-grade adults. Only half of the high-grade affectives responded, however, and a third of this group were, in fact, made worse.

TABLE III

	Worse	No Change	Some Improve- ment	Much Improve- ment
	(Per cent.)	(Per cent.)	(Per cent.)	(Per cent.)
<i>Children:</i>				
Epileptics (6)	17	50	33	0 (33% responded)
Psychotic-schizophrenics (1) ..	—	100	—	— (no response)
Affectives (6)	0	17	33	50 (83% responded)
Simple hyperactives (8) ..	0	13	50	37 (87% responded)
<i>Low-grade Adults:</i>				
Epileptics (15)	6	20	47	27 (74% responded)
Psychotic-schizophrenics (5) ..	0	60	40	0 (40% responded)
Affectives (4)	0	0	25	75 (100% responded)
Simple hyperactives (18) ..	17	22	22	39 (61% responded)
<i>High-grade Adults:</i>				
Epileptics (7)	14	29	14	43 (57% responded)
Psychotic-schizophrenics (6) ..	0	66	17	17 (34% responded)
Affectives (6)	33	17	17	33 (50% responded)
Simple hyperactives (6) ..	0	0	17	83 (100% responded)

COMPLICATIONS

Only two children showed side-effects, one being a little drowsy, and the other complaining of generalized "pins-and-needles" with transient erythema. Both continued on an unaltered dose.

Of the adults, five had slight drowsiness not requiring an alteration in dose, but 20 showed more severe effects, 6 having to be taken off Stemetil and 13 clearing up on a reduced dose.

TABLE IV

	Low-Grade	High-Grade
Rigidity and tremor	5	1
Excessively drowsy	6	2
Severe worsening of behaviour	1	3
Dizziness	—	1
Headaches (transient)	—	1
	12	8
	—	—

The average weight of those patients showing severe complications was 9 stones, the same as for the test group as a whole, but it will be seen from Table IV that, whereas the distribution of low-grade to high-grade is approximately the same as in the total group, there is a predominance of drowsiness and extra-pyramidal effects in the low-grade complications.

DISCUSSION

The results as a whole show that 65 per cent. of the patients derived benefit from Stemetil, and in some cases this was considerable, such as one hyperactive low-grade who was able to wear normal clothes for the first time in years. In several cases there had been no previous response to a variety of other tranquillizers, and it is significant that in nine cases the nursing staff spontaneously asked for the patients to be put back on the drug shortly after the trial ceased. Similarly, the School Supervisor reported that all her children showed better concentration and progress, this being in agreement with the findings of Segal and Tansley (6) using hydroxyzine, though the trial was too short to produce a significant alteration in I.Q.

It will be seen that the schizophrenic group was the most resistant to any effect, 67 per cent. showing no change. None was made worse, however. The epileptic groups also did not do much better, none of the epileptic children being "much improved" and 17 per cent. being worse, the adults following a similar, though less well-marked, pattern. This is in contrast to the findings of Bergin and Bergin (7) using promazine. The proportionately better effect amongst the low-grade epileptics might be ascribed to the greater general incidence of epilepsy amongst this grade.

The most effective general response was seen in the affectives and simple-hyperactives in all grades, with the possible exception of the affective high-grades. This might be due to the difficulties of differential diagnosis from other psychotic overlays at this intellectual level, however, and it should be remarked in general that sub-dividing mental defectives into reaction-types can only be tentative and not assertive. The useful value of tranquillizers in hyperactive defectives is in agreement with the several other authors mentioned above, but not with the negative findings of Gillie (8) and Craft (9, 10). In view of the known

action of Stemetil it would be expected that it would have its best effect in the hyperactive and affective (particularly agitated) groups.

When the dosage and complications of Stemetil are considered, it will be seen that there is a predominance of low-grades amongst those showing extra-pyramidal effects and drowsiness, on relatively high doses. This is in accordance with the known general effect that relatively smaller amounts of drugs acting on the central nervous system may be required for the smaller physical amounts of such tissue functioning in low-grade defectives. In this respect it appears that weight is less of a guide to dosage than intelligence, when these extremes are considered.

In general, it would seem that Stemetil has a real value in alleviating behaviour disorders in mental deficiency practice, and thus facilitating the training and education of patients who would not otherwise function at their full capacity. It seems of particular value in the affective-hyperactive groups of patients of all grades and ages, but it is necessary that a finer grading of personality and reaction-types be achieved before the worsening of some patients can be avoided. This appears worthy of further study.

SUMMARY

A new tranquillizer, prochlorperazine ("Stemetil"), was given for a trial period of two months to 88 mental defectives of all grades and ages. Sixty-five per cent. of them showed appreciable behaviour improvement. The simple hyperactives and those with affective disturbances gave the best response. Defectives with schizophrenic overlay proved the most resistant, and epileptics showed a variable response, being better with the low-grade adults.

Extra-pyramidal and sedative complications occurred mainly in a proportion of the low-grade adults, and all the side-effects were transitory, disappearing with diminished dosage.

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SOME RECENT CRITICISMS OF THE DIMENSIONAL ANALYSIS OF PERSONALITY

By

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IN recent issues of THE JOURNAL OF MENTAL SCIENCE there have appeared two critical papers (5, 7) dealing with some aspects of the dimensional analysis of personality which the writer has put forward. As replies to these papers separately would seem to require a good deal of repetition, it seemed better to frame a joint reply. This has been kept rather short on purpose, primarily because the writer does not believe that arguments are very helpful to the advancement of science, unless they are accompanied by new data of an experimental kind. In the main, therefore, this reply has restricted itself to simply pointing out that many of the points raised are factually incorrect, or, although they might be correct in themselves, are not relevant to the theory they are criticizing.

To begin with we may deal briefly with a paper by Hamilton (5), who reports obtaining 15 sets of results from 11 tests, which for some unstated reason he appears to consider as measures of one and the same personality trait; he does not give any correlational data to support this view. Apparently 12 of these 15 scores differentiate between his neurotic and his normal control subjects; in addition significant differences were obtained between various neurotic groups. Most numerous are the differences between anxiety states and hysterics (seven); least numerous those between obsessionals and hysterics (three); the number of differences between anxiety states and obsessionals (six) is intermediate. Hamilton considered these results to be "at variance with the conceptual experimental and statistical framework that is usually associated with the work of Eysenck". He seems to base his conclusion on two implicit hypotheses: 1. the tests used by him are measures of extraversion-introversion, and 2. the measures used are relevant to the theoretical analysis made by me of this concept. If this were so then indeed we might be mildly surprised that the number of significant differences between obsessionals and hysterics is not larger than it is, obsessionals usually being grouped with anxiety states as part of the dysthymic group. However, neither hypothesis is tenable. The theory of extraversion-introversion in terms of the excitation-inhibition balance, which I have advanced (2), does not permit of any predictions with respect to the majority of the tests used by Hamilton, and indeed he makes no effort to show that any such deductions can be made. It follows that the results can only have the most tangential relevance to the theory I have suggested. In the second place, Hamilton's own results show quite clearly that his tests are tests of *neuroticism* rather than of extraversion-introversion; it will be remembered that nearly all his tests differentiate significantly between normals and neurotics. This suggests the possibility that his various neurotic groups may have differed with respect to degree of neuroticism. The very perfunctory analysis of the data given by Hamilton makes it impossible to discuss his results any further; it

would have been necessary for him to have carried out a factor analysis and a canonical variate analysis before any conclusions at all could have been drawn from his experiment; even then, of course, it would have been better if he had chosen for his experiment tests relevant to the theory of extraversion-introversion. It is always difficult to disprove a theory when the tests used are irrelevant to that theory!

In addition to his experimental study, Hamilton criticizes my theoretical framework. His criticism here appears to be based on a fallacy, namely that the system of classification which I have put forward is one involving categorical groups. Thus Hamilton talks about "Eysenck's scheme of classifying psychological groups into dysthymics, hysterics and psychopaths", and states that my investigations "would appear to represent the substitution of one system of mutually exclusive classification of clinical types for another". This, of course, bears no relation to my actual theory as put forward in "Dimensions of Personality" and later publications. I propose there two main continua or dimensions, neuroticism and extraversion—introversion, to account for a large proportion of the behaviour characteristics of non-psychotic human beings. I have shown that as a matter of fact individuals high on neuroticism and extraversion tend to be labelled *psychopaths* by psychiatrists, while subjects high on neuroticism and on introversion tend to be labelled *anxiety states*. There are several other resemblances between the categorical method of classification of the psychiatrist, and the continuous dimensional method suggested by myself. These correlations are of some practical and theoretical interest but they do not convert my system into one of categorical classification, a notion against which I have argued on many occasions. Hamilton seems to be under the mistaken impression, as illustrated in his figures 3 and 4, that arbitrary changes in the metric of a dimensional analysis are relevant to this issue; this is too obviously incorrect to deserve a lengthy refutation. His argument is formally equivalent to stating that a change in the system of labelling longitudes, which would shift their origin from Greenwich to San Francisco, would alter our weather! As most of the remainder of Hamilton's criticism appears to be based on this fundamental error, there appears to be little point in continuing this reply.

The article by Storms (7) reanalyses certain data collected by Hildebrand (6) and uses in doing so the very methods which Hamilton ought to have used in the analysis of his own data. The result is an interesting comparison of different multivariate analyses tending to show, as one might have expected, that discriminant function analysis gives results which are not in all points identical with the results of factor analysis. Storms' arguments and mathematical developments are quite correct, and I would fully agree with his conclusion "that adherence to dimensions derived from factor analysis when analysing differences among groups can lead to serious loss of information, and may lead to inefficiency in practical applications or oversimplification in theoretical interpretations". Interesting as this demonstration may be, it is difficult to see why Storms should imagine that these results in any way contradict my theory. Indeed, Storms seems to realize this when he admits that "Eysenck will not find anything inconsistent in the finding of several principal components in such analysis, since he does not claim that his psychoticism, neuroticism, and extraversion-introversion dimensions cover the whole range of human variation". Storms appears to devote the rest of this paragraph to saying that if I had made such a claim his results would have disproved it; a point which would hardly seem even of academic interest, since few people would ever have

imagined that all the variabilities of human behaviour could be accounted for by just three dimensions!

Storms states that I imply "that the important differences among neurotic groups are accounted for by different degrees of extraversion". This statement is correct or incorrect, depending on the interpretation one chooses to put upon the word "important". I believe that a consistent theory of extraversion-introversion in terms of inherited differences in the excitation-inhibition balance can account for certain important symptom patterns, behaviour patterns, and test score patterns characterizing the main nosological groups recognized by classical psychiatry in the field of the neurotic disorders. The demonstration that other factors discriminate between the groups, and that the correspondence between clinical diagnosis and position on the extraversion-introversion dimension is far from perfect, is implicit in my theory. I would be willing to argue that from the scientific point of view the systematic differences corresponding to my theory and predictable from it are more important than the purely empirical findings having no psychological rationale which Storms reports. I am quite willing to agree that this use of the word "important" implies a subjective value judgment based on my reading of the history of science, which suggests the importance of theoretical considerations and the hypothetico-deductive method.

A similar dubiety attaches to the use of the word "practical" in Storms' belief that "adherence to dimensions derived from factor analysis . . . may lead to inefficiency in practical applications". By this he presumably means that if we wanted to discriminate between the diagnostic groups used by Hildebrand on the basis of the tests administered by him, factor scores would be less useful than scores based on discriminant function analysis. This is quite likely true, although one would have liked to have seen a cross-validation study applying both sets of formulae to sets of groups other than those from which they were derived. However that may be, I can see little practical point in using a long and complex battery of tests for the simple purpose of giving a diagnostic judgment which more or less efficiently approximates that given by the psychiatrist in charge of the patient, and obtainable with much less trouble. As I have pointed out before, I think there is some interest in showing that groups of psychiatrically diagnosed patients have certain predictable positions on the dimensions into which I have analysed certain aspects of personality; this, however, does not mean that the purpose of this analysis lies in the simple duplication of the psychiatrists' efforts. I believe that categorical segregation of patients into diagnostic groups is erroneous theoretically, and historically explainable in terms of a faulty analogy with the qualitatively different disease processes usually found in physical disorders (3). My purpose is to substitute for this a dimensional analysis describing each individual patient in terms of his position on a number of relevant continua, rather than in terms of separate categories. I would not regard the "practical" purposes which Storms is referring to as having any real practical value at all, and I would, therefore, regard his mathematical solution as being of relatively little use to the practising psychiatrist even from the practical point of view. From the theoretical point of view, his results lack significance because at no point do they make contact with psychological or psychiatric theory.

In all this I do not wish to deny the great value which discriminant function analysis may have when used in the right context. There are certain theoretical problems which can probably be solved by the use of these techniques, and indeed several such uses have been reported from our laboratory (1, 2, 4). What

does seem to be relatively pointless, however, is the use of complex statistical techniques for their own sake and without any recognizable theoretical context which could be clarified by their use. However perfect such applications may be technically, they do not advance their subject matter.

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A DAY HOSPITAL FOR NEUROTICS IN AN INDUSTRIAL COMMUNITY

By

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THE Uffculme Day Hospital was opened some two and a half years ago as part of an experimental scheme to treat psychiatric patients outside the confines of a mental hospital. The plan was to admit a small number of patients and gradually to develop and organize a group therapeutic technique in the light of experience. It was hoped that a Day Hospital conveniently situated within a large industrial city would help to meet an increasing need for diagnostic and therapeutic facilities. It might find a solution for those patients who could not be adequately treated as out-patients and yet were not likely to benefit from admission to already hard-pressed and overcrowded mental hospitals. While the present Day Hospital scheme is too small to have a significant influence on the total psychiatric problems of some one million and a half people living in the vicinity, it has so far shown signs of being a welcome addition to the mental health services of the region.

Some of the principles and practice of day hospitals have been discussed by Bierer (1951); Barnard *et al.* (1952); Aron and Smith (1953); Moll (1953); Cameron (1947); Harris (1957); Cosin (1954); Craft (1957) and others. It is clear that day hospitals vary considerably according to local needs and the interests of the psychiatrists concerned. While it would be premature to draw far-reaching conclusions from the Uffculme Day Hospital, the experience gained in evolving a special type of day hospital and in using group psychotherapeutic methods may be of interest to others planning similar accommodation.

ACCOMMODATION AND NUMBER OF PATIENTS

The Day Hospital is located in a large mansion type of house which also contains an out-patient department and in-patient unit. The only special accommodation for the day hospital comprises an assembly room from which the patients radiate to their daily programme of activities. The occupational therapy department, a large room used for relaxation therapy and physical training, a group treatment room, and a dining room are shared with the In-patients' Department; but the daily time table ensures alternating use of these facilities by the Day Hospital and In-patients, thus preserving the identity of the Day Hospital as a group unit.

At present the Day Hospital comprises 16 patients of both sexes. The decision to limit the number to sixteen patients was made not only for physical

reasons of available space but also from psychological experience in group formation. Sixteen was found to be about the maximum number to be adequately treated in a single group which maintains its identity without too much splitting, isolation or diffusion. Large numbers were found to split up into little cliques and to foster individuals who isolated themselves without identifying themselves with the group. The greatest danger of larger groups is the formation of a diffuse mob which may be excessively suggestible at one moment or stubborn and resistive at another. In it patients can easily lose their individuality, and the group atmosphere depends too much on one leader or the person in charge and not on the interpersonal relationship to which each member of the group contributes his share.

It was felt that the Day Hospital, as a group, operated best during the early stages when the numbers were around ten. Pressure of admission made it uneconomical to limit the number below sixteen.

Undoubtedly, Day Hospitals can be satisfactorily run with numbers well in excess of those accommodated at Uffculme; but in our view a satisfactory group regime would not be operable with much larger numbers—except by dividing the patients into several groups with arrangements to ensure that each preserved its own identity. This is well in accord with the pedagogic experience that much can be done in education with a small group of say fifteen, much less with a group of thirty and little if anything with a group over fifty.

CRITERIA FOR ADMISSION

The guiding principle was to admit only those patients whose condition would otherwise require full in-patient hospital treatment. Admissions were selective and directed mainly towards cases of severe and chronic neurosis; they included many patients who had failed to respond to out-patient psychotherapy and had not returned to work. A number had been in-patients in a mental hospital and a small number were admitted for a transitional phase during which a patient's transfer from hospital to home could be made easier.

From the first it was decided to concentrate on patients during their active working life by excluding patients over sixty and under sixteen. Special Day Hospitals for the elderly seem most desirable, but represent an entirely different task. Of particular importance in the Uffculme experiment was the criterion of occupational adaptation; only in very special circumstances were patients removed from work to enter the Day Hospital, and absence from work or, in the case of housewives, inability to cope with household duties, were taken as a positive reason for admission. Patients with severe behaviour disorders, marked aggressive or offensive tendencies or assessed as suicidal risks were considered unsuitable; likewise, patients who by reason of physical disease or very limited intelligence were unlikely to benefit from the regime were excluded.

It was originally hoped that admissions could be arranged in such a way as to produce a "balanced" group. This balance would involve approximately equal numbers of both sexes and a group-"personality" with fair mixture of traits, such as introversion-extraversion and optimism-pessimism. In practice it was not possible to achieve this aim and the group-"personality" fluctuated considerably. It was, however, soon found that too many autistic personalities and withdrawn or blunted schizophrenics prevented satisfactory group formation even if their behaviour was entirely unimpaired and their superficial collaboration seemed perfect. Subsequently the admission of this type of patient was limited so that the numbers did not exceed three of the total of sixteen

patients. Remitted schizophrenics often have the desire to mix freely in spite of their very limited capacity for empathy. It was found to be important to restrict their number.

The admission of certain other types of patient had most beneficial effects on the group as a whole; as one would expect, a patient whose acute symptoms improved rapidly under treatment exuded an atmosphere of optimism. Some patients, in particular certain hysterics and psychopaths, appeared to have a catalytic effect on the group and speeded up interpersonal reactions with apparent therapeutic results. This catalytic effect was often independent of group leadership. At times when the group seemed to be in the doldrums, a new admission of the right personality type could transform the therapeutic situation.

The principles applied in selection of admissions with the purpose of a balanced group formation remain up to the present ill-defined and understood; nevertheless, one of the authors (J.A.H.) who interviewed all patients before admission developed a sense of those patients who might fit into the current group. His predictions were often wrong and some patients who had been confidently selected as being valuable additions to the group did not live up to this expectation; others who were admitted with doubts as to their ability to settle proved to be sources of strength and stability within the group. It will be a future scientific project to develop more insight into the principles of such selection. Foulkes' suggestion (personal communication) that patients for group treatment are best selected by a group interview deserves attention.

AIMS AND MEANS

The primary aim of the Day Hospital was to provide a short intensive period of group psychotherapy as part of a programme of social rehabilitation for all patients, with individual psychotherapeutic and physical treatments where necessary. It was decided to limit the *period of treatment in the Day Hospital* arbitrarily to *three months* and to adhere to this without being inflexible if the need arose. Patients were told before admission that their stay would not exceed this period and therefore accepted discharge without rancour even if there had been little or no improvement. The decision to adopt a time limit has not been regretted; it has prevented some of the very longstanding neurotics from becoming permanent patients; it has given patients of better prognosis a definite time in which to work towards recovery. The time limit gives a patient a goal and, more important for the neurotic, a time span in which he is motivated to deal with his problems before facing reality again. While the time limit as such does not solve transference problems or dependency needs, it has the important function of preventing iatrogenic prolongation of neurosis.

A not insignificant number of patients who improved rapidly in the Day Hospital re-developed symptoms just before discharge. This was interpreted as fear of discharge rather than an indication for a further or indefinite period of treatment. Patients who were minimally improved or unchanged were told that they had probably derived the maximum benefit from this form of treatment and that further stay would be unlikely to help them. Most of the patients who stayed longer than the time allotted were retained because arrangements for their return to work were not completed and it was felt that a period of inactivity at home awaiting a job would upset a precarious balance.

In order to facilitate treatment in the group much emphasis was placed on the creation of a flexible and self-contained *therapeutic community* in which patients could sense a warm and friendly atmosphere; a situation where there

was freedom for self expression and security from rebuke. While rules and prohibitions were kept to a minimum and spontaneous group activity encouraged, the unit is not anti-authoritarian in the sense that staff authority is deliberately minimized. Authority is in the background, permissive and flexible, but firm if the need arises. In our view the concept of denigrating authority for therapeutic purposes, as practised in some hospitals and units dealing with a similar clientele, defeats its own aims if it is carried to the extent of insisting that all patients and staff should be on exactly equal terms. A completely equalizing regime makes both patients and staff insecure and encourages excessive acting-out. There are many intermediary possibilities between the equalizing of staff and patients and the carefully preserved hierarchy system revealed in the hospital analysed by Stanton and Schwartz (1954). Such an intermediary approach was found useful in the Day Hospital.

STAFFING

A high staff ratio is regarded as essential. *Medical* staffing consists of three consultant sessions and a registrar who devotes about half of his time to the unit. The bulk of medical time is devoted to group and individual psychotherapy. *Nursing* staff consists of a male charge nurse who is responsible for day to day administration and routine nursing work but spends most of his time conducting relaxation and physical recreation classes for Day Hospital and In-patients. This nurse spends about half of his time with the day patients and is assisted for short periods by a female nurse from the Out-patient Department. One *Occupational Therapist* occupies a key role spending about two-thirds of her time with the Day Hospital patients. She has responsibility for social and recreational activities, including art therapy, dancing, play reading, etc., in addition to occupational therapy of the conventional kind. The much-needed services of a *Social Worker* have recently been obtained, though previously because of difficulties in providing a Social Worker home visits had been carried out by nursing staff. A senior *Clinical Psychologist* carries out routine intelligence and other tests and also takes part in the group psychotherapy. His most important role is in *vocational guidance*; he is most valuable through his help in finding work and re-employment of patients through his knowledge of local industries and his intimate contact with Ministry of Labour Officers and D.R.O.s.

A weekly *staff conference* is attended by all members of the Day Hospital staff and serves as a channel of staff communication and forum for the discussion of administrative and therapeutic problems.

INVESTIGATIONS

Observation of behaviour within the group is a valuable diagnostic aid and in several cases a somewhat obscure diagnostic problem has become clear after a few weeks observation in the group. It has been notable how often neurotic patients present an entirely different picture in an individual interview with the doctor when compared with behaviour in the group. The hysteric not infrequently appears depressed and full of complaints when interviewed; in the group the same patient is often seen to be laughing and apparently free of discomfort. This *normalizing effect of the group* is quite striking in some cases and often indicates a less serious level of disturbance than had first been thought.

Early autistic features and incongruous mood changes in schizophrenia, previously masked by neurotic symptoms, became quite apparent in the group

and aided diagnosis. Apart from the value of the group as a help in diagnosis, observation of interpersonal relationships, interests and attitudes in the group give the psychiatrist a lifelike and more objective appraisal of the patient's personality. It has often been said that psychiatrists spend too much time in the artificial atmosphere of the consulting room and too little in a more natural social setting in which he can make a more life-like assessment of his patients. Group life in a Day Hospital serves such a purpose well and experience over the past two years has shown that a little time spent with the Day Hospital in its various activities is amply repaid. Observations on individual and group behaviour are made by all the staff and the results pooled at the weekly staff conference.

In addition to routine psychiatric and physical examination special investigations such as EEG, X-rays, and pathological tests are available where indicated. All patients have routine intelligence tests and special psychological testing where necessary. Social case work and home visiting by the Social Worker form a necessary part of the treatment.

PROGRAMME OF ACTIVITIES

It was made a principle that patients should be fully occupied from the moment of arrival (9.0–9.30 a.m.) to the moment of departure (4.45–5.0 p.m.). To achieve this a daily routine time table was developed. Although some flexibility was applied, all patients were expected to take a full part in the routine and a patient was only excused from some activity if there were good reasons for this. The result is in some ways not unlike a school time table; it consists of alternating periods of occupational therapy, physical training, relaxation therapy, group psychotherapy, games, dancing and social recreation. There are weekly periods for art therapy, play reading and regular outings to places of interest. The period devoted to each of these activities is generally an hour, so that there is a balanced alternation between physical and mental activity and relaxation. The lunch hour constitutes a free period when patients can choose what they wish to do; some go to the local shops, others read or play cards and some prefer to sit and do nothing.

One afternoon a week is devoted to *special activities* suggested by the group themselves; this may consist of an outing to the cinema, a party game or a walk in the local park. While individual or group initiative is encouraged and the patients' suggestions always heeded, it has not been found that leaving the patients to their own devices and expecting them to organize their activities themselves works well or is therapeutic. This *laissez-faire* approach praised by some therapists was found to encourage a state of inertia in the group from which very few patients could stimulate the rest into activity.

As previously mentioned the group situation fluctuated considerably according to the nature of the patients; at times active leadership emerged within the group and the group developed its own momentum and required little stimulation or interference from the staff; at other times the opposite state prevailed; there was little group interaction or activity unless guidance came from the staff. While in general the staff approach to the group was not intended to be inspirational, a group without inspiration or activity was not regarded as therapeutic. When such a state of affairs existed a more active approach from the staff was encouraged. In order to maintain the identity of the group the staff were specifically instructed and encouraged to see individuals as part of the group, and to regard each group activity as part of the whole group situation.

Two features of the daily programme deserve more detailed mention:

Relaxation Therapy

The decision to introduce this form of treatment was based on the experience of the authors while working in the Crichton Royal Hospital, Dumfries, where Mrs. Gertrude Heller had introduced her method and practised it with remarkable success in a variety of patients. It is based on the simple theory that many neurotics complain of subjective feelings of tension often localized in various parts of the body and that these sensations can be abolished by regularly repeated lessons and exercises in relaxation. While the details of the method cannot be described here two aspects need emphasis: by these exercises in the group the patient acts out his surrender to the therapeutic situation. This surrender is made easy for him—only very few patients have so far protested and refused to take part.

The natural control and economy of movement which is the basis of Mrs. Heller's method is accepted by nearly all patients as something they need and the logic of being *taught* to relax both mind and body is something they can understand.

Group Psychotherapy

Group psychotherapy sessions are held *daily* for all patients attending the Day Hospital in a *special group room*. The sessions, lasting one hour, are conducted alternately by two senior psychiatrists and the senior clinical psychologist. The groups are "open" in that the population gradually changes composition. These group discussions are regarded as an important part of the whole treatment within the Day Hospital and not as something sacred and apart from everything else.

The group sessions have been approached with few preconceived notions or theories apart from the principle that the therapist should be relatively passive and inactive, encouraging the group to spontaneous communication. Much interpretation is done by the group itself and the conductor's role varies slightly according to the therapist taking the group on that particular day. The use of three different therapists for the same group may have theoretical drawbacks but in practice there has been little evidence of its disadvantages for a group of this type. The alternation of the conductors seems to give the group its own momentum and prevents rigidity of style. It appears to encourage intergroup relationships and to prevent a group which is too much centred on the therapist. While the theoretical understanding of such a group is open to a host of interpretations, some functions of the group are readily discerned: The sessions have a broad *educational function*; patients learn many things they did not know before; they see the *other patients have problems similar* to their own, the solution and understanding of which may throw light on their difficulties. The group allows *expression of emotional attitudes* and thereby has a cathartic effect. The group also has an important *social function* by supporting its members and by normalizing distorted attitudes and prejudices.

While some aspect of health education is frequently a starting point of discussion in the group, there is no planned lecturing or schedule of instruction on the cause of neurosis as was once practised by Maxwell Jones in his Mill Hill group therapy. Each discussion generally ranges far and wide, perhaps starting with the work of the housewife deflecting to the problem of bringing up children then changing abruptly to life after death and perhaps finally swinging into a political topic or narrowing into the causes of headaches. If marked tension and

controversy arises in the discussion of religion or politics, the conductor usually encourages the whole group to come out into the open and express their personal attitudes even if there is little agreement. Sometimes, but not very often, the argument becomes acrimonious and repetitive and it is necessary to suggest a change of topic without resolving the dissension.

As can be seen from the themes just enumerated, discussion does not always centre around the neurosis itself and is often far removed from personal psychological problems. At times the free nature of the discussions may be used by the group to avoid matters of a personal nature; e.g. when the Russians launched their Sputnik a whole group session was filled with repetitions of what members had read in the newspaper. Even here it was not felt to be necessary to disturb the group's response to an event of great consequence. Only if the discussion took the form of empty gossip about women's attire or football pools between two or three group members was it necessary for the conductor to deflect the conversation into one more stimulating for the rest of the group.

While the content of the group discussions appears to be broadly similar with the same group under the three alternating conductors; there is a suggestion that more subtle variations of content exist and that these are in the direction of the individual beliefs and expectations of the conductor taking the group on that particular day. One patient illustrated this point rather vividly in a different context. He had been treated in a psychotherapeutic group at another hospital some years previously and remarked that in the previous group the discussion constantly returned to the topic of sexuality at the behest of the conductor. While acknowledging the value of these focused groups for his own problems he admitted that the wider discussion method was helping him to understand his attitude to other problems.

In a group of sixteen it is not easy to get all members to contribute to the discussion. Provided the patient listens with attention it is not in our view essential for him to verbalize in the group to derive benefit. From individual interviews with some of these silent listeners it is clear that not only those who talk can profit from the discussion. Nevertheless, it is undesirable that the group has a majority of listeners dominated by a few who are only too willing to talk and usurp the whole conversation. Under such circumstances the listeners become bored and distracted and it is necessary for the conductor to intervene and encourage all members to contribute to the point under discussion. With a group of sixteen there is a notable danger of the conversation being carried on in several simultaneous dialogues or an undercurrent of whispers. This danger appears to be lessened by getting the group closely together round a large table.

Hysterics and similar neurotics with histrionic or exhibitionistic tendencies have sometimes a disturbing effect and provoke such hostility or anxiety in the group that tactful restraint by the conductor is needed to restore equilibrium. Schizophrenics, on the other hand, tend to be taciturn and lacking in empathy for the atmosphere of the discussion. They often interject something beside the point which tends to halt the free flow of talk, but occasionally a remark of pseudo-profound half-sense can enliven the conversation unexpectedly.

The presence of more than a third of schizophrenics in a mixed neurotic group has a disruptive effect which is not easy to explain. From the experience which one of us (W.M.-G.) had with discussion groups consisting only of schizophrenics undergoing insulin treatment, the following observations may be of interest. The atmosphere of affective coolness in the majority of group members was often very marked; nothing seemed to arouse any warm interest

or sustained attention, whatever was being discussed. Tactless or cruel remarks by individuals were frequent and were strongly resented by others in the group, who subsequently refused to attend. Callousness side by side with oversensitivity in the early schizophrenic seemed to break up the group before it could be formed. When such schizophrenics are well diluted in a neurotic group, the forces which hold the group together and preserve its morale generally prevail.

On the whole the group sessions are not very popular and the resistance of its members is shown by being late for the group sessions or by trying to find excuses for absence. For the same reason intervals of silence are not infrequent. While it is sometimes helpful to sit out such silences and allow group tension to accumulate until somebody feels compelled to start talking, there are occasions when the conductor has to suggest a way round the resistance by suggesting a topic that may stimulate interest. Fortunately many of the group sessions are lively affairs with little or no need for the conductor to intervene or direct; such discussions are often carried over into conversation at meal times and give the impression of having made some impact on the individuals within the group. Just how much the discussions contribute to the result of day hospital treatment is impossible to gauge at present.

SOCIAL CLUB

A weekly social club is held on Thursday evenings and run by an elected committee of patients. Attendance is entirely voluntary and there is no official subscription, though patients may subscribe to the club funds if they wish. Membership of the club is automatic for all Day Hospital patients both current and discharged. There is *no time limit* in connection with the club, so that a patient may start attending the club while under active treatment at the Day Hospital and continue for as long as he wishes afterwards. A smaller number of out-patients attend the club by invitation at the request of their doctor. Each member of the club is encouraged to bring a relative or friend.

The evening's programme of activity commences at 7.30 and ends at 10 p.m. and is held in the large waiting hall and adjoining Day Hospital centre. Refreshments are prepared by the Day Hospital patients in the Occupational Therapy Department. The evening's programme is previously planned for each week by the committee and run by them with any help they request from the staff. The activities are wide, and include party games, card games and dancing. Liberal time is allowed for patients to gossip with their friends during refreshment time. On average about forty to fifty people attend the club, though this number doubles on special occasions. Overcrowding has not yet been found to be a problem.

The social club has been found to be an *indispensable part of the Day Hospital*. One of its most important functions is to ease the patient's discharge and give continued group support for as long as the patient needs it. Many patients form strong friendships in the Day Hospital and the club enables them to derive advantages from these relationships.

The club is always attended by a senior psychiatrist and the Occupational Therapist; their presence there is felt to be essential to the club's success.

While individual interviews are discouraged, the psychiatrist spends part of the evening chatting with the patients, particularly those who have been discharged. It has been found that one evening a week at the social club saves hours of out-patient time for the psychiatrist giving "supportive therapy". A

few words from the doctor and a gossip with friends seems to provide some chronic neurotics with the "prop" that they need.

Discharged Day Hospital patients are not given further out-patient appointments unless specially indicated. They know that the doctor is always at the club and they can have a short word with him during refreshments. For many patients the club has provided a practical answer to unresolved transference and dependency needs which are spread within the social group at the club.

The club also provides a valuable *follow-up clinic* with little expenditure of time and energy from the doctor, who can observe discharged patients for as long as they continue to attend.

As far as our observation goes, most patients attend the social club for two or three months after discharge and then drop off in attendance. Some patients reappear at the club after several months of absence; in a few this indicates the first sign of a relapse while in others its meaning is not clear beyond the impression that the patient again feels the need for a social group's support and encouragement.

PRELIMINARY RESULTS

The Day Hospital is included in a specially designed statistical scheme for the recording of information in all parts of the Clinic.

The first *hundred admissions* to the Day Hospital have been analysed for certain data relevant to this discussion. Analysis of other findings will only be of interest after they have been accumulated in a larger number of cases.

Of these 100 patients, 48 male and 52 female, analysis by age groups showed the males to be fairly evenly distributed between the ages of 17 to 55 years while the females showed a definite peak distribution, exactly half of them being between the age of thirty and forty.

The majority of patients were referred to the Day Hospital by consultant psychiatrists, 19 came from general practitioners, 5 from mental hospitals and the remainder from various social agencies. Thirty-three patients had at one time of their life been treated as in-patients in a mental hospital and thirteen of these had repeated mental hospital admissions. Only 14 patients had no psychiatric treatment prior to admission to the Day Hospital.

Of the first 2,000 possible daily attendances, there were 1,789 actual attendances. This attendance percentage of 89 per cent. is not accounted for by part-time patients, who are not normally admitted. Housewives may need to absent themselves for a few days during the illness of a child, etc., but in most patients absenteeism or unpunctuality was without good excuse and was interpreted as a feature of neurotic maladaptation which might respond to treatment. For this reason the group of 16 was seldom at full strength.

Analysis of the family history of the cases showed that 26 patients had parents and/or siblings and/or blood relatives who had received treatment for psychiatric illness. No consanguinity among the patients' parents was found.

Table I shows the *principal psychiatric diagnoses*; the admissions consisted mainly of psychoneurotics, with only fifteen psychotics who were remitted or in the process of remission.

The admission rate for all categories of depressive illness of 29 per cent. compares with 65 per cent. for the Maudsley Day Hospital and 57 per cent. for the Bristol Day Hospital (Craft, 1957)—undoubtedly influenced by age and other criteria in selection.

TABLE I
Principal Psychiatric Diagnosis

Schizophrenic disorders	9
Manic depressive disorders	6
Anxiety reactions	18
Hysterical reactions	15
Phobic reactions	8
Obsessive-compulsive reactions	6
Neurotic depressive reactions	23
Mixed psychoneuroses	4
Psychosomatic reactions	2
Pathological personality	9
	100

(12 patients had "pathological personality" and a further 12 "immature personality" as a subsidiary to the principal psychiatric diagnosis)

Table II shows the rated *results on discharge*; about one-third showed only slight improvement, were unchanged or deteriorated; the remainder were regarded as having shown a significant change in the direction of recovery. While improvements on the whole were in accordance with predictions before treatment there were a few notable dramatic responses which were not expected.

TABLE II
State on Discharge

	Male	Female	Total
Recovered	1	6	7
Much improved	11	15	26
Improved	14	13	27
Slightly improved	14	7	21
Unchanged	6	4	10
Deteriorated	2	2	4
Lapsed	2	3	5

The percentage of patients who lapsed in their day hospital treatment against medical advice was 5 per cent. compared with 15.7 per cent. reported for the Maudsley Day Hospital and 13.4 per cent. for the Bristol Day Hospital (Craft, 1957).

Three patients required transfer to a mental hospital while under treatment and a further three to a general hospital for treatment of an accompanying physical disorder.

The transfer rate of 6 per cent. was lower than in the Menninger Day Hospital (16 per cent.), the Bristol Day Hospital (10.4 per cent.), and the Maudsley Day Hospital (14.7 per cent.). This may well have been due to different selection of patients.

One patient made a demonstrative suicidal attempt, but continued treatment in the Day Hospital; another (schizophrenic) patient lapsed in her attendance and was "found drowned" a week later; this patient had been advised to have E.C.T. but had refused treatment.

Of particular interest were thirty patients who were *persistently unemployed* before treatment; fifteen of these returned to gainful employment following treatment, the others remaining unemployed after discharge. In these cases the change of attitude to employment taking place while day patients seemed of far greater importance for return to work than the degree of psychiatric disability.

Considering the results of day hospital treatment in relation to diagnosis, the results in the obsessive-compulsive disorders were the most disappointing; phobic reactions on the other hand were more responsive to treatment though severe cases of this kind sometimes required longer than the routine period of three months. A few psychopaths did surprisingly well.

Apart from the group and individual psychotherapy described, about two-thirds of the patients were given symptomatic drug treatment; two were specially treated by hypnosis and fifteen received courses of E.C.T. while in the Day Hospital.

SUMMARY AND CONCLUSIONS

The organization and treatment programme in a Day Hospital for severe and chronic neurotics is described. Group methods of a particular type have been applied and the value and limitations of such methods assessed. The first hundred admissions have been analysed for data relevant to preliminary results.

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SENSORY DEPRIVATION AND SCHIZOPHRENIA

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MUCH interest has been displayed in the past few years in the effect on human subjects of reduction, or as far as possible, abolition of sensory stimulation, so that virtual isolation from the environment is produced. Recent comprehensive reviews have appeared, dealing with experimental work (Solomon *et al.* 1957) and conditions arising incidentally in the course of various therapeutic procedures (Grünthal 1957), and it is therefore unnecessary to deal with the topic at length here.

It might be hoped that exposure of schizophrenic patients to sensory deprivation would shed some light on the nature of their illness. A widely held view is that schizophrenia is a state in which there is excessive internal mental activity, this interfering with the normal interaction between the organism and the environment and preventing appropriate behavioural responses to the outside world. The frequent use of such terms as autism, withdrawal, retreat from reality, lack of reality testing and internal pre-occupation imply some such concept. Autistic phenomena in the shape of visual hallucinations and "waking dreams" appear in normal subjects exposed to sensory deprivation, although only after fairly long periods in most cases. A reasonable hypothesis would be therefore, that schizophrenic subjects would be more susceptible than normal subjects to the effects of sensory deprivation and would respond to it with a worsening of their mental state.

On the other hand some of the phenomena of schizophrenia have been explained on the grounds of an inability to deal with the inflowing stream of sensations and percepts and to extract meaning from them, a more passive type of failure. One would not expect sensory deprivation to aggravate this process; it might even produce improvement by reducing the strain and bringing temporary relief to the mechanism involved.

With these considerations in mind 12 schizophrenic subjects were placed in conditions similar to those used by Bexton, Heron and Scott (1954) aiming at an absence of patterning or the reduction of stimulation to a point at which perception is no longer possible. They lay on a couch in a soundproof cubicle, wearing opaque goggles and gloves fitted with long cardboard cuffs extending downwards over the tips of the fingers. They were left in the cubicle for half an hour in the first instance, then on another day for a period up to two hours. One patient whose reactions were particularly interesting went in for two further periods of 3 hours each. It seemed unjustifiable to interrupt therapeutic activities for longer. Any patient who asked to do so was taken out without argument or attempt to persuade him to stay in.

Whereas most normal subjects find the experience of sensory deprivation unpleasant, overestimate the time that they have been in the cubicle and need substantial inducements to be persuaded to stay in it (Solomon *et al.* 1958), the schizophrenics tolerated it remarkably well. All completed the

preliminary half hour without protest. Only five did not complete the second two-hour period and of these only two felt real distress, a girl who had a strong erotic hallucination and a young man who conceived the idea that an extra pair of gloves which were lying in a corner of the cubicle would fly at his throat and throttle him. Two were bored and anxious to carry on with more amusing activities and the fifth was a restless deluded woman who wanted to talk about her persecutors. The more fatuous hebephrenic type of patients liked being in the cubicle, referred to it as restful and peaceful, were reluctant to come out, and usually grossly under-estimated the length of time that they had been in it. In fact all the patients under-estimated the time that they had been in the cubicle, even those who asked to come out. The only effect on the symptoms noted was restricted to the period actually spent in the cubicle and to the sphere of hallucinations, which tended to be less vivid and troublesome. This was true of auditory and to a lesser extent of tactile hallucinations. The one patient who had had visual hallucinations did not experience them in the cubicle. One 32-year-old woman who had been distracted by voices in her head with *écho des pensées* for some three years reported that they stopped while she was in the cubicle and volunteered the remark "they must have been all my imagination", something that she had never said before. They started again as soon as she came out of the cubicle, however, and on subsequent occasions she experienced occasional auditory hallucinations in the cubicle. Her general mental state showed no change, in spite of some 8½ hours in the cubicle altogether. Another woman of 52 who had heard persistent and distressing voices for ten years said that in the cubicle they were fewer and less of a babel, so that they were easier to put up with, although they still threatened and said unpleasant things. Patients with illnesses of more recent onset reported cessation of the voices while they were in the cubicle. In all cases no substantial change in the patients' mental state occurred in the week during which they were spending periods in it. The girl who had an hallucination of being raped while in it had many such in the ordinary ward.

This effect on hallucinations accords well with clinical findings in organic delirious states, in which it has been noticed that patients are less prone to visual hallucinations when in a room with plain white walls than in one with patterned wallpaper, that plain curtains are less disturbing to them than multi-coloured ones and so on. It seems that the hallucinations occurring in mental disease differ sharply from those produced by sensory deprivation in normal subjects in regard to the conditions which elicit or suppress them. From the effect on auditory hallucinations it appears that normal sensory stimuli reinforce hallucinatory experiences in schizophrenia, in contrast to the effect of visual deprivation in mentally healthy individuals which can produce visual hallucinations. These observations give no support to the concept of schizophrenia as a welling up of spontaneous mental activity of the same sort as the release phenomena postulated by Hughlings Jackson.

This tendency of the patients to underestimate time in the cubicle prompted an enquiry into their performance in this respect in ordinary ward conditions. Six schizophrenic patients were interviewed, returned to the dayroom of the ward and then asked half-an-hour later how long they thought it was since they had been talking with the interviewer. Only one made an approximately correct reply and it turned out that he had been doing occupational therapy sitting in front of a clock in the interval. Of the others, one gave the time as 5 minutes, 2 as 10 minutes, 1 as 15 minutes and 1 as 20

minutes. This trend therefore appears to be general and not confined to conditions of sensory deprivation. Llanan and Goldstone (1956) found that schizophrenics tended to overestimate the length of time intervals of approximately one-second duration. The conditions of their experiment are so very different from this one that the two results do not necessarily conflict. Time estimation is a complicated subject which still remains to be investigated adequately.

SUMMARY

Twelve schizophrenic patients were placed in conditions of sensory deprivation for short periods. They were more tolerant than normal subjects of these conditions, which seemed to reduce the intensity of hallucinations.

I am indebted to the British Broadcasting Corporation for the gift of a soundproof cubicle which had been used in one of their programmes, to the House Governor and Chief Engineer of Bethlem Royal Hospital and the Maudsley Hospital for its erection in a disused padded room and to Doctors Cohen and Penfold for help with the observations.

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THE TEMPORAL RELIABILITY OF THE MAUDSLEY PERSONALITY INVENTORY

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THE Maudsley Personality Inventory (MPI) that purports to assess the two personality dimensions, extraversion-introversion (E-I) and neuroticism-normality (N-N), has been used extensively in recent research work and a number of published papers in somewhat different spheres bear witness to the use made of the questionnaire (Eysenck, 1956a; Coppen, 1958; Franks, 1957; Bartholomew, Franks and Marley, 1958). A clear description of its construction and use has been presented by Eysenck (1956b) and a comparison between it and other personality scales, as well as some criticisms of its construction, has been set out by Jensen (1958). The particular interest this questionnaire has evoked is due in part to the fact that it is one of the few personality inventories constructed and standardized this side of the Atlantic. The importance of a reliable questionnaire of this type is noted by Eysenck (1958) in his attempt to produce a shortened version of the MPI that will be of value in such work as market research.

So far the questionnaire has been investigated for internal reliability in terms of the split half correlation (Eysenck, 1956b). However, it is desirable that its temporal reliability (or temporal stability, Anastasi, 1956) should be known, particularly as it has been considered that the two personality variables remain fairly constant for any one subject (Eysenck and Prell, 1951; McLeod, 1953; Blewett, 1953; Franks, 1954). To remedy this deficiency the following test-retest investigation was carried out.

METHOD

Over the past two years a number of subjects were requested to complete the MPI as part of various research projects. All subjects were considered as being "Normal", neurotic or psychopathic and no one was included with a diagnosis of mental deficiency, psychosis or gross brain damage. As many of these subjects as could be traced and who had completed the questionnaire at least eighteen months previously were sent the MPI by post and again invited to complete it.

The replies were scored in the same way as on the previous occasion (an increment of one for an answer in conformity with the key and 0 for an answer not in conformity: $\frac{1}{2}$ was scored for an answer indicating uncertainty) and a coefficient of correlation was calculated for both personality scales.

The replies were divided into two groups formed with reference to previous

research carried out with different populations. The first group consisted of subjects who were suffering from various neurotic disorders, "normal" controls, and first offenders being treated in an out-patient clinic. The second group consisted of neurotic subjects from a psychotherapy ward and recidivists investigated in prison.

RESULTS

Group 1 contained 50 subjects (29 males and 21 females) with an average age of 29·6 years and a range of 21–58 years. There were 28 neurotics, 14 "normals" and 8 first offenders. Group 2 contained 40 subjects (27 males and 13 females) with an average age of 31·4 years and a range of 20–64 years. There were 13 psychotherapy patients and 27 recidivists.

These 90 subjects that were traced and replied represented 63·4 per cent. of the original population tested (142 subjects).

The findings are set out in Tables I and II.

TABLE I

The Means, Standard Deviations and Coefficients of Correlation Found on Test-Retest in Group 1

	Mean	..	Test 12·49	Retest 13·01	Correlation +0·735
E-I	S.D.	..	4·61	4·93	($p < 0·001$)
	Mean	..	14·04	12·14	+0·621
N-N	S.D.	..	5·96	5·46	($p < 0·001$)

TABLE II

The Means, Standard Deviations and Coefficients of Correlation Found on Test-Retest in Group 2

		..	Test	Retest	Correlation
E-I	Mean	..	12·15	12·47	+0·749
	S.D.	..	4·80	5·72	($p < 0·001$)
	Mean	..	14·85	14·07	+0·797
N-N	S.D.	..	5·64	6·76	($p < 0·001$)

DISCUSSION

It can be seen that the MPI does achieve a high degree of temporal reliability and on these grounds can be considered a valuable research tool. The coefficient of correlation would have approximated even more closely to unity were it not for the few subjects who produced marked differences in their test-retest scores. These persons tended to be those who when first seen were diagnosed as hysterics or hysterical psychopaths.

In this small group the greatest degree of concordance was noted in those subjects that had acted as normal controls but their numbers were too small to justify any statistical treatment.

The neurotics, who had for the most part left hospital, and in many cases psychiatric support, at the time of retest, showed a marked concordance, but it is not possible to say whether, in fact, they are cured or better. It can, however,

be stated that a number of them returned the questionnaire with a letter suggesting in many cases they were markedly improved as compared with the first testing in hospital. The tentative inference is that treatment and subsequent improvement in mental health do not greatly affect response to this personality questionnaire.

There were no marked differences on test-retest between the criminal and the non-criminal populations, which again suggests that treatment in hospital has little effect on questionnaire response.

The lie scale included in the questionnaire was not investigated but did not appear to be of value even in those cases where marked test-retest discrepancy occurred. The lie score was as frequently small in those subjects with test-retest discrepancy as it was high in subjects demonstrating a high degree of concordance.

SUMMARY

The value of the MPI as a research tool is alluded to.

A test-retest investigation into the temporal reliability of the questionnaire is described.

It is concluded that the MPI has a satisfactory temporal reliability in relation to both personality dimensions, E-I and N-N. This reliability appears unaffected by hospital treatment and imprisonment.

The views expressed in this paper are our own and do not necessarily represent the opinion of the prison commissioners.

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PREMEDICATION FOR E.C.T. WITH MEPROBAMATE

By

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It is a well-known fact that patients dislike E.C.T. and that part of this dislike is based on the unpleasant period of waiting which occurs before all operative procedures. There may be indeed other features involved in E.C.T. as patients seem to have a fear and dislike of this treatment out of proportion to what would be expected. That this dislike of E.C.T. is an important matter is brought out by the difficulty in persuading patients who have suffered a recurrence of a depressive illness to undergo this treatment once again, and if any means could be devised to increase their readiness to accept medical advice on this subject then an advance would be made.

As a means of lessening the anxiety and tension present during the waiting period prior to this treatment it was decided to try one of the new tranquillizers, namely meprobamate, as a pre-operative treatment for E.C.T. The experiment also had a second purpose in that it put the tranquillizer itself on trial and tested its efficacy in combating a constant source of anxiety, namely the waiting period for electrical treatment. Both in-patients and out-patients were used in this experiment and the only criteria used was whether or not the patient would be able to sensibly answer certain questions at the end of treatment. The patients were randomized according to the first letter of their surnames; one group received 800 mg. of meprobamate one hour before E.C.T. and the control series received two tablets of an inactive compound; the nursing staff were asked to give an evasive answer when asked by the patients about the purpose of the tablets and to confine their replies to such remarks as "it's to help the treatment work better", etc. All E.C.T. given was modified by an intravenous anaesthetic and a relaxant.

Some time after the cessation of the course of treatment the patients were sent a questionnaire and asked to state how much they disliked having the treatment; whether they would undergo treatment again if advised to do so; what was the worst part of the treatment; and whether the tablets taken before treatment helped in any way to act as a calming agent. There were 50 patients in the control group and 70 in the tranquillizer group.

The results were as follows:

Did you dislike having electrical treatment?

		Control	Tranquillizer
Very much		8 16%	17 24%
A little		21 42%	23 33%
Not at all		21 42%	30 43%

There is a very slight suggestion that patients having Meprobamate disliked treatment more than those on the control tablets, but the difference is quite insignificant.

The next question dealt with degree of readiness to undergo the treatment a second time if so advised:

The answers were:

				<i>Control</i>	<i>Tranquillizer</i>
(a)	Without hesitation	..	..	23 46%	41 59%
(b)	Reluctantly	..	..	21 42%	17 24%
(c)	Very reluctantly indeed	..	..	1 2%	9 13%
(d)	In no circumstances	..	..	5 10%	3 4%

Grouping the two most unfavourable results together gives a χ^2 with 2 degrees of freedom = 4.269, ($0.2 > P > 0.1$), which is not significant.

If one compares those who would have treatment for the second time against those who would not, then results are slightly favourable to the tranquillizer group but are not significant.

What was the worst part of the treatment?

				<i>Control</i>	<i>Tranquillizer</i>
(a)	Waiting to have it	..	..	30 60%	37 53%
(b)	Coming round after it	..	..	16 32%	25 36%
(c)	All of it	..	..	1 2%	1 1%
(d)	None of it	..	..	3 6%	7 10%

There was no significant difference.

Did the tablets you received before treatment help to calm you?

				<i>Control</i>	<i>Tranquillizer</i>
(a)	Yes	..	..	36 72%	58 83%
(b)	No	..	..	14 28%	12 17%

Here χ^2 (with one degree of freedom and continuity connection) = 1.437 ($0.3 > P > 0.2$).

Although the tranquillizing drug appears to be slightly more effective than the inactive tablets the result is not significant.

CONCLUSION

It was not shown that the pre-treatment administration of meprobamate as against inactive tablets significantly diminished the dislike of treatment or increased readiness to undergo a further course of E.C.T.

The impression received was that those patients who made a good response to E.C.T. disliked it least and were prepared to have it again with the minimum of hesitation.

PSYCHOSIS IN ASSOCIATION WITH HAEMOPHILIA

By

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IN spite of the vast literature concerning Haemophilia, I have been unable to find any reference to the concomitant occurrence of a psychosis with this condition.

Schachter (1948) reported briefly on the psychological aspect of the disease in a six-year-old boy.

There are brief allusions to confusion and convulsions following cerebral accidents. Arbuse and Locascio (1937) report headache, semicoma, transient hemiplegia, mental deterioration and psychoses in chronic types of subdural haemorrhage. They also describe mental irritability or excitement followed by hebétude and persistent headache, vomiting, delirium and unilateral convulsions in cases of chronic subdural haematoma.

Aggeler and Lucia (1944), reviewing C.N.S. complications, refer to intracerebral accidents which led to convulsions, confusion, restlessness, irritability and defective memory and to headache as a presenting symptom followed by apathy, stupor and coma.

Douglas and McAlpine (1956) in a recent paper on neurological complications of haemophilia and Christmas Disease describe a case of hemiplegia of 41 years' standing who at the age of 10 lost the power of speech, which returned to normal by the age of 14 years. No psychiatric disability was reported.

This paper reports the occurrence of a severe depressive illness in a haemophiliac of 50 years of age:

The patient was born on 14 August, 1905. He was admitted to the Bristol Mental Hospital on 17.2.56.

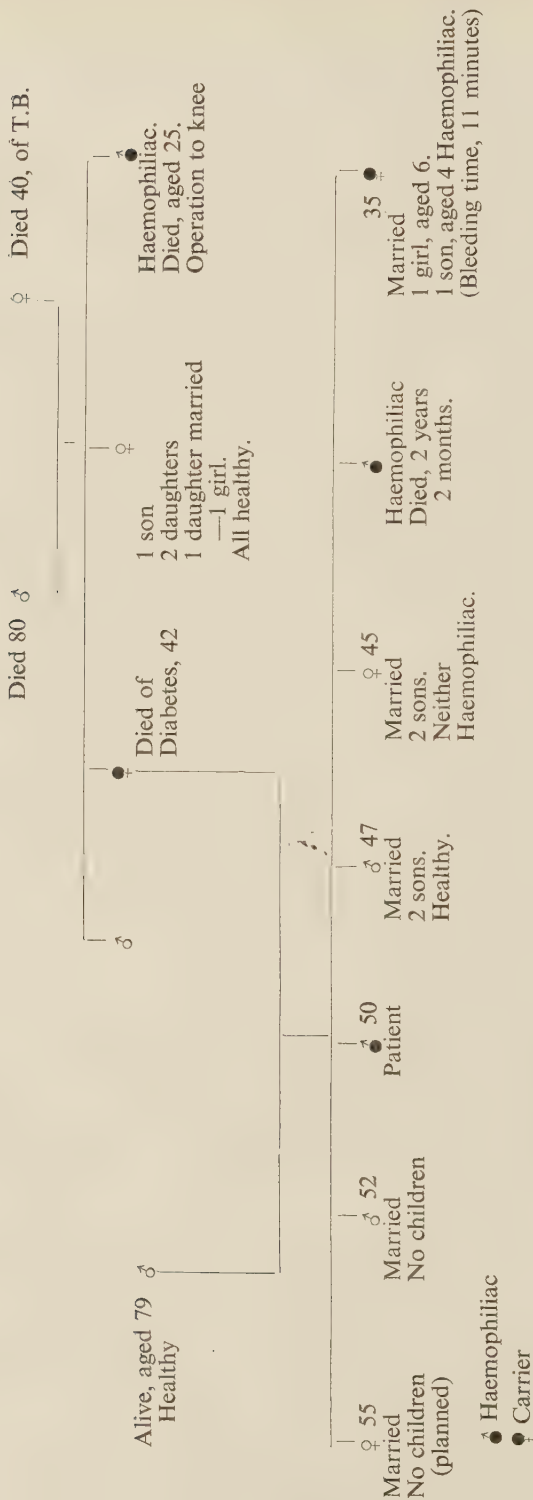
His mother was a diabetic who died at the age of 42. One brother was a haemophiliac and a sister's son is known to be a haemophiliac (see Family Tree).

He bruised easily when an infant and had had many haemorrhages in childhood and adolescence, including a prolonged haemorrhage into his scrotum, nose bleeding, frequent bleeding into elbow joints, knee joints, wrists, shoulder joints and left hip joint. These haemorrhages usually followed trivial injury such as a twist or a slip off the pavement on to the road. He had also bled twice from his gastro-intestinal tract and on about a dozen times from his urinary tract.

In youth he was intelligent and "quick at learning". His career at secondary school was broken by his illness and he left at 16 without taking school certificate. Until the age of 20 he worked as a railway clerk. Because of his condition he has not been able to do paid work since (1925). He was, however, fairly active on his home farm until the age of 41, since when haemarthroses of his joints has progressively restricted his activity. On admission he had restricted movement at all major joints except the right hip joint and had had a recent effusion into his left elbow. He suffered from dental caries. Otherwise he was in reasonable physical health. Three days after admission he had a mild spontaneous haematuria, which cleared in three days. At that time his bleeding time was 3 minutes, clotting time 16 minutes, platelet count was 159,000 per c.mm.

Mentally: Two weeks before admission he had spent 5 days in another mental hospital which he left unimproved against advice. Three weeks before this he had a haemorrhage into a knee joint which necessitated bed treatment. During this time he became deeply depressed, lost all interest and complained of lack of energy. He did not sleep for 4 nights and he told

FAMILY TREE



his family that his mind was gone, said that he had been very wicked. He feared that he might harm his sister and her children aged 6 and 4 years. On two occasions he became exasperated and shouted out "Oh God, let me die".

On admission to this hospital he was still classically depressed. He said that he had lost interest in everything, was unable to concentrate and could not sleep without sedation. He felt "guilty about everything—even about being a haemophiliac". He said that he could not get out of his mind a feeling that he had committed a sin for which he could not be forgiven. He reluctantly related a minor sexual peccadillo which occurred when he was 15 and said that this thought continued to recur. He complained of stubborn constipation. He felt that he was "going out of his mind".

His case was discussed with the Regional Transfusion Officer who advised the avoidance of any manoeuvre which might possibly cause trauma. He was therefore treated conservatively and initially he improved superficially. Three weeks after admission he again became depressed. One week later, at 4.30 a.m., he was discovered by the night nurse hidden under the bedclothes, scraping at his left wrist with the top of a tin. The bleeding was controlled and he was transferred to the Bristol Royal Infirmary for repair. He was returned to the Bristol Mental Hospital after 3 days, his wound having been sutured. A week after his return to us, he had to be re-admitted to the general hospital because of a pulsating haematoma at the site of his wound. This was removed and the wound re-sutured. It subsequently healed uneventfully.

Mentally he remained depressed and it was decided to give him a course of E.C.T.

Initially he was given E.C.T. under cover of 2 pints of fresh frozen plasma administered just prior to treatment. Atropine gr. 1/100 was given a half an hour beforehand and the convulsion was modified by pentothal 0.5 g. and scoline 100 mg. Relaxation, which was satisfactory, was carried out by a consultant anaesthetist. E.C.T. was repeated on the following day, whilst the patient was still under the cover of the plasma administered on the day previously. Modification was so satisfactory that further administration of plasma was considered unnecessary. A course of seven treatments by E.C.T. was given: the quantity of scoline necessary to modify the fit was gradually increased to 200 mg. All treatments were uneventful and the patient made a complete recovery. Two years after cessation of treatment, he remains well mentally.

DISCUSSION

Because of the severity and frequency of bleeding in a haemophiliac, an attempt was made to treat a severe depression conservatively. This resulted in an apparent initial improvement in his mental condition, but the fact that the improvement was apparent rather than real was proven by the early occurrence of a determined suicidal attempt. This forced us to decide on balance of risks to proceed with E.C.T., the treatment which would have been clearly indicated were it not for his severe disability.

Slight trauma had in the past caused quite severe bleeding and this rendered E.C.T. technically difficult. His first two treatments were given under cover of 2 pints of fresh frozen plasma, modified with pentothal and scoline and both administered within 24 hours. In this way, each treatment was covered by one transfusion. Modification was sufficiently successful to allow us to give further E.C.T. without transfusion. This was helpful, because it had become very difficult to secure a suitable vein for transfusion. It was found necessary, however, to increase the dose of scoline to 200 mg. in his last treatment to secure adequate relaxation.

CONCLUSION

This case illustrates a technical difficulty in giving E.C.T. to a haemophiliac who previously had had quite severe haemorrhages following minor traumata.

SUMMARY

A case of depression in a haemophiliac is described.

No similar case has been found in the literature.

The difficulties of treatment and the technique of overcoming them are discussed.

ACKNOWLEDGMENTS

My thanks are due to Dr. T. Wilton who administered the initial two anaesthetics and to his Senior Registrar, Dr. Betty Slee, who completed the course for me; to Dr. G. H. Tovey, Regional Transfusion Officer for his advice and to Dr. Frederick Bolton who supervised the transfusion "cover".

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A STUDY OF THE USE OF AZACYCLONAL HYDROCHLORIDE ("FRENQUEL") IN CHRONIC SCHIZOPHRENIA

By

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IN February, 1955, Howard D. Fabing described in *Science* (Fabing, 1955a) a clinical study using the drug azacyclonal hydrochloride (Frenquel), alpha-(4-piperidyl) benzhydrol hydrochloride. This drug is the gamma-isomer of Meratran which, although chemically similar, alpha-(2-piperidyl) benzhydrol hydrochloride, has a rather different action. Brown and Werner (unpublished but referred to by Fabing, 1955a) reported that Frenquel prevents or diminishes central stimulation induced in the mouse by various agents, whereas Meratran has a stimulant action.

Later the same year Fabing (1955b) reported that model psychoses produced by LSD-25 and by mescaline can be blocked by Frenquel, and further, that in one of the experimental subjects an LSD-25 psychosis was brought to an abrupt end by the intravenous injection of this drug.

In discussing the therapeutic value of Frenquel, Fabing wrote, "Some cases of acute schizophrenia, alcoholic hallucinosis, senile and arteriosclerotic hallucinosis, and, to a lesser extent, some of the more chronic schizophrenic syndromes, respond to the oral administration of this drug to a degree that has encouraged us to continue our observations, which will be reported later". In November, 1955, Fabing and Hawkins reported clinical details of its use in certain syndromes and we were particularly interested to note that of their group of 34 cases of chronic schizophrenia, 8 cases made "Full Adjustment" on Frenquel only, and a further 8 made "Full Adjustment" on Frenquel with either chlorpromazine or E.C.T. or both.

The problem of chronic schizophrenia is one of such great importance that treatment effecting such good results must be pursued further. We noted that Rinaldi, Rudy and Himwich (1955) reported on the use of Frenquel in the "Treatment of Disturbed Patients with Psychosis of Long Duration". The great majority of their 39 cases were suffering from a chronic schizophrenic illness, and 21 of them were reported as showing definite improvement with this treatment. They were given doses of 10 mg. b.d. over an 8-week period; 14 of them showed improvement by the end of the second week, and the other 7 at the end of the fourth week.

Clark (1956) reported the use of the drug in schizophrenia (including 5 cases of chronic schizophrenia) without benefit, and he further failed to confirm reports of the blocking of LSD-25 psychosis by premedication with this drug,

or that it exerts a specific suppressive effect when given intravenously at the height of intoxication. Forster and Henderson (1957) found no significant changes with doses of up to 400 mg. daily by mouth for an average duration of 49 days in a small series of 18 chronic schizophrenics who were visually and constantly hallucinated.

In this country Sargent (1956) in his article on Chemical Tranquillizers, made a preliminary report on the use of Frenquel on sixteen cases of schizophrenia of various types, stating:

“However, some schizophrenics seem to have had their tension damped down with Frenquel alone, while others improve when Frenquel is combined with E.C.T. after it has failed on its own.”

A study of the very limited literature available has clearly shown that controlled experimental work was needed to establish the position of this drug in the treatment of chronic schizophrenia. Accordingly we arranged and conducted the following experiment.

CASE MATERIAL

A total of 60 cases made up of 30 females and 30 males were chosen for this therapeutic trial from the long stay wards of this hospital (a mental hospital of approximately 900 beds). Fifty-eight of the cases completed the trial, one female patient being withdrawn due to physical illness and one male patient was too unco-operative to continue.

All were considered to be suffering from a chronic schizophrenic illness. The duration of hospitalization in this series of cases varied from 3 to 34 years. The average duration since first hospitalization was 13½ years. Auditory hallucinations were a prominent symptom in almost all the cases and in addition they all showed some degree of behavioural disturbance and personality deterioration.

CLINICAL ASSESSMENT

One investigator (C.F.L.) carried out the clinical assessments of all cases by means of an interview and with the use of a rating scale. The cases were assessed prior to the experiment, at the end of the first period, and final assessments were made at the end of the second period. In addition, weekly progress reports were specially submitted in all cases by the nurses in charge of the wards where the patients resided.

The Rating Scale (illustrated in Table I) was devised to measure 9 important characteristics in the subjects of chronic schizophrenia, and each of these was measured initially on a six-point scale. Each characteristic and the total picture was reassessed at second and third interviews and results were tabulated on the basis of “Improved”, “Deteriorated” or “No change”.

TABLE I

1. Appearance	0 to 5	6. Attention	0 to 5
2. Motor activity	0 to 5	7. Behaviour	0 to 5
3. Thought form	0 to 5	8. Habits	0 to 5
4. Thought content	0 to 5	9. Hallucinations	0 to 5
5. Affect	0 to 5		
0=“Very disturbed”.		5=“Normal”.	

Today the importance of adequate control and assessment of drug trials is well recognized. In this trial the principle was adopted by which neither the

patient nor the assessor knew whether the drug or a placebo was being administered. From our previous experience with drug trials we felt it inadvisable to disclose that one of the drugs in use was a placebo and so both drug and placebo were given pseudonyms.

DOSAGE AND DURATION OF TRIAL

The report of improvement by Rinaldi, Rudy and Himwich of 21 of their 38 cases showed 14 of them improved at the end of the second week and the other 7 at the end of the fourth week. The dosage used in their trial was 20 mg. daily. Literature from the firm issuing Frenquel recommended that in the acute reactions in schizophrenia the usual effective dose is 40 mg. three to four times daily.

Having considered these factors we decided to give each case in the series a four weeks trial with a daily dose of 120 mg. given as 40 mg. t.d.s., and a four weeks trial with a placebo identical in appearance. The cases were divided into two groups and, following a short period of drug abstinence, one group was given azacyclonal hydrochloride (Frenquel)—referred to during the rest of this trial report as azacyclonal—40 mg. (2 tablets) t.d.s. The other group received the identical placebo, again as 2 tablets t.d.s. Following a short break at the end of the first four-week period, the two groups were changed over. The organization of the groups and distribution of the drugs was in the control of the one of us (L.J.L.) not involved in the assessments, with the aid of our pharmacist.

RESULTS

The results of this trial are annotated in Table II.

			TABLE II		
No. of Cases			Improvement	Deterioration	No Change
Azacyclonol	..	58	4	10	44
Placebo	..	58	4	7	47

COMMENT

The striking feature of these results is that in the great majority of cases (44 out of 58), azacyclonal, in the dosage used, had no effect on these patients suffering from a chronic schizophrenic disorder. It should be pointed out that 2 of the 10 cases which deteriorated while on azacyclonal, and 5 of the 7 which deteriorated while on the placebo, had, prior to the commencement of this project, been having either chlorpromazine or reserpine, both of which we have found of value in the treatment of chronic schizophrenia, and their removal may have been a factor in the worsening of these cases. However it was our impression that three of the cases which deteriorated while on azacyclonal appeared to show a general marked aggravation of their schizophrenic illness, and their behaviour became so disordered that in two patients the drug had to be discontinued for the rest of that section of the trial. This impression is in keeping with that of Gray and Forrest (1958) who, in their recent article, state that it is their clinical impression that the drug had a central stimulant action. We found no clinical evidence of toxicity with azacyclonal during this trial.

ANTI-HALLUCINATORY ACTION

Particular attention was paid during this trial to the influence of azacyclonal on the auditory hallucinations of this series of chronic schizophrenic cases. Assessment of results on this aspect are annotated in Table III.

TABLE III

		No. of Cases with Auditory Hallucinations	Improvement	Deterioration	No Change
Azacyclonal	..	56	1	7	48
Placebo	..	56	3	3	50

COMMENT

The salient feature is again that the great majority of this series, 48 out of 56, showed no change in the intensity of their auditory hallucinations. One case showed a mild diminution in the intensity of auditory hallucinations, but of the 7 showing aggravation, the same 3 who showed great deterioration of their behaviour on azacyclonal, also showed a marked intensifying of their auditory hallucinations.

SUMMARY

A controlled clinical trial of the use of azacyclonal hydrochloride (Frenquel) in a series of 58 chronic cases of schizophrenia is reported. The results of this survey suggest that this drug, in the doses used in this trial, has no place in the treatment of chronic schizophrenia, and in addition, this trial suggests that it has no specific anti-hallucinatory action with regard to auditory hallucinations in this disorder.

ACKNOWLEDGMENTS

We would like to thank Dr. G. Fitzpatrick for his help and interest in this trial, the nursing staff and medical secretaries for their invaluable assistance, and our Pharmacists for their kind co-operation.

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RESERPINE AND CHRONIC PSYCHOSIS: TWO-YEAR OUTCOME IN A TREATMENT GROUP

By

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IN the last few years a number of new pharmacological agents have been introduced into psychiatry and reserpine has been one of the most controversial of these. Reports have varied from the over-enthusiastic (Noce, Williams and Rapaport, 1954), to the cautiously optimistic (Moore, 1956) and even the frankly sceptical (Cutler, Monroe and Anderson, 1957). It would be true to say, however, that all mental hospitals use reserpine now to some extent and nearly all psychiatrists are agreed that some remarkable recoveries have occurred. However, many questions remain to be answered—What kind of case improves?—What is the nature of the improvement qualitatively?—How lasting is the effect? Time alone gives the answer to some of these questions which are constantly recurring phenomena in therapeutics.

To take crude examples, penicillin has fully justified all that was claimed for it, while cortisone has signally failed to do so. How much can one expect from the “tranquillizers”? The treatment of chronic patients in the mental hospital is a searching test for any drug, for these patients have all received previous treatment of varying types which has failed.

The present report gives the outcome of two years of treatment in a group of 40 psychotic women. They had all been ill for a number of years (range 3–23 years, mean 8·4). All were severely disturbed mentally, necessitating closed ward care, though not actually classed as refractory patients. This group of 40 was selected from a ward population of 73 and the only factors governing selection were (1) that there had been a recognizable personality present previously, i.e. the individual had made some sort of reasonably successful work and social adjustment prior to the illness, and (2) that their behaviour was such as to represent a burden to the nursing staff (eating habits, noisiness, poor personal standards of hygiene, etc.). Nursing staff remains critically short in numbers in the mental hospital and the long-stay wards often have to be sacrificed to the more acute treatment wards in terms of staffing, so the genuine attempts which are being made to raise the social matrix of such wards are repeatedly frustrated by the absence of sufficient personnel. Behavioural improvement in a substantial group would lighten the load on the nurse in the chronic ward and allow her to be something more than a hygiene-supervisor-cum-invigilator. To all intents and purposes these 40 patients were condemned to a lifetime in a mental hospital.

The distribution by diagnostic classification was as follows:

TABLE I					
Paraphrenia	Paranoid Schizophrenia	Schizophrenia Unclassified	Catatonic	Hebephrenic	Chronic Mania
8	13	12	3	2	2

Treatment began in December, 1955. Dosage in the individual cases varied considerably and changed from time to time depending on symptomatology. Nine to ten mg. was the average daily starting dose, the maximum reached was 16 mg. and the tendency was for the dose to be gradually reduced so that those who still continue on treatment are receiving 3-6 mg. daily, which is always raised if there is any hint of recrudescence.

RESULTS

The two-year results are summarized in Table II.

TABLE II
Outcome, in 40 Chronic Patients, of Two Years' Reserpine Treatment

Discharged		Still in Hospital				
Home and Working (? cured)	Home and Doing Some Work	Deaths	Re- admitted	Maximum Hospital Status	Temporary Improve- ment Only	Failed to Respond
5	8	1	Nil	6	9	11

Home and Working

Five have resumed full social status (12 per cent.), and may be tentatively labelled "cures". One is working as a bus conductress (a catatonic who was mute and doubly incontinent for eight years), one is working in a laundry (ill for seven years, leucotomized five years previously), and three have resumed their roles as mother of the household with full responsibility (ill 8, 8 and 7 years).

Home and "Adjusted"

Eight patients (20 per cent.) are categorized under this heading. They all show some personality defect in some way and the chances are that they probably could not support an independent existence. Four are passively docile and occupy a similar position in the home to that of a child or an old person. They do some simple housework and some cooking. Two still have paranoid ideas but are managing within the bounds of social acceptance. The remaining two are still living mainly in the world of fantasy although at home with their family but have sufficient reality adaptation to make this a feasible proposition for their relatives. As a general principle patients were only discharged to tolerant families, no forcible discharges took place.

Returns

None of the thirteen discharged patients have so far been re-admitted.

Deaths After Discharge

One patient hanged herself after being at home for three months. She was a single woman of 70 who lived with her sister and suffered from paraphrenia. Was this misfortune due to a reserpine-induced depression? The evidence is against this explanation. She was a Christian Scientist who, despite promises, stopped taking the drug once she was at home. Her "healer" also persuaded her to continue, for the hallucinatory shouting which had been such a feature of the case had stopped. However the intense conflict of principle appeared to have overwhelmed the patient.

Maximum Hospital Status

Six patients (15 per cent.) have full ground and town privileges, work in the hospital setting and live in the highest (sociologically speaking) female open door ward with no supervision. Three have completed Rehabilitation Courses at a Ministry of Labour establishment nearby. Of this sub-group it is felt that four could easily live at home save that during the long illness (14 years in one case) homes have broken up and disappeared and there is no place for them to go.

Failures

Twenty patients are classed as having failed to respond to treatment (50 per cent.). Nevertheless, of the twenty, nine (22·5 per cent.) did make substantial and appreciable improvement to begin with. Poor personal hygiene, asocial eating habits, gaudy overdressing and loud hallucinatory shouting may be given as types of symptoms which improved under treatment. Since social isolation tended to be complete in this group (i.e. none, or very few, visitors came to see them) it was regretfully decided after one year to allow them to slip back into their psychosis, since further rehabilitation would have led to a situation of greater expectation which could never have been fulfilled. They have been classed as “temporarily improved”.

There remained a hard core of eleven patients (27·5 per cent.) in whom no significant improvement could be detected after two years’ treatment.

Management After Discharge

Of the thirteen discharged patients, ten attend out-patient clinics monthly for assessment and further supplies of the drug. Three are unwilling drug takers and have to be cajoled and disciplined by family or health visitor into taking tablets. Two patients need no further treatment and one has stopped of her own accord. Without exception the families realize fully the heavy dependence of all concerned on the drug and co-operate in its administration. They fear a return of symptoms and appreciate the increased docility which the drug produces in the patient. When one bears in mind how suspicious the psychotic is in taking any medicine, it is remarkable that seven of the ten are eager and reliable in their continued use of the drug to avoid relapse which they fear.

Other Factors Influencing Outcome

In an uncontrolled situation like this it is impossible to know all the factors at work—placebo response effect, atmosphere in which treatment is given, etc. (Trouton, 1957); with these reservations, however, a few obvious considerations spring to mind and some impressions may be ventured.

TABLE III
Factors Influencing Outcome

					Yes	No
Personality deterioration	..	..	..	..	—	+
Length of illness	..	..	..	..	—	+
Diagnosis	..	..	..	..	—	+
Age of onset	..	..	..	..	+	
Degree of family cohesion	..	..	..	..	+	

Degree of apparent personality deterioration, length of illness and diagnosis had little influence on the outcome—surprising recoveries occurred in apparently dilapidated personalities. Age of onset was important though: an illness developing for the first time in a person of twenty-five or more was more likely to respond to treatment. Most striking of all the factors was, however, the degree of family cohesion (see Table IV).

TABLE IV
Mean Annual Number of Visitors in the Various Groups

Mean Annual Number of Times Visited	1956	1957
Discharged group	45	—
Maximum hospital improvement group	12	20
Failed group	17	13

Social isolation is already recognized as a powerful factor associated with mental breakdown (see, e.g. Hare, 1956). It may be that it is also an important determinant as to which patients will respond to treatment.

Length of Time Since Discharge

The total number of months discharged in these thirteen patients is 121, with a mean individual discharge time of 9.3 months, as against a total illness time of 82 years with a mean individual hospital stay of 6.3 years. For time distribution of discharges see Table V.

TABLE V
Length of Time Thirteen Discharged Patients have Remained Out of Hospital

Length of Time Discharged	3-6 Months	6-9 Months	9-12 Months	Over 12 Months
Number of patients	4	3	4	2

Comparison with Other Groups

Crude comparisons may be gained from consideration of total hospital discharges of chronic patients in previous years. Table VI shows the number of patients who were discharged after a hospital stay of more than 3 years in the years 1953-57.

TABLE VI
Total Hospital Discharges (of Patients with More than 3 Years' Hospitalization) in the Years 1953-57

1953	9
1954	17
1955	13
1956	12
1957	28

The figure of 28 discharges in 1957 includes 11 from the group here reported.

DISCUSSION

If this can be considered a typical result of reserpine treatment it seems to signify that a not inconsiderable proportion of chronic psychosis is reclaimable, even up to 10 or more years of illness. The results are not "pure" however in the sense that a very active group-social-work programme was instituted in the second year for those patients who showed increased "awareness" on the drug

regime alone. Such a programme was not originally intended, but since the effects of the drug were to make the patient more accessible, more sociable and much more verbal and conscious of her environment, the change was forced upon the ward organization on humanistic grounds by the patient's re-awakening needs. The mute and statuesque catatonic demands little enough attention, but a lively and restless patient, repeatedly asking "When can I go home?" demands a great deal of attention. As Moore pointed out (*ibid.*) the tranquillizers mean more work for the mental hospital, not less.

It is not yet clear how long treatment must continue. In most cases it has been necessary for 2 years and it may have to be envisaged on a permanent basis.

A vague motor restlessness—pacing, repetitive movements, motor mannerisms, etc.—is almost always present, associated with some muscular weakness. Depression has not been encountered in the present series though it has occurred rarely in personally treated cases outwith the present group and must be accepted as a risk (Wallace, 1955). The same remarks apply to cardiac decompensation, to which Marley and Pare (1956) were quick to draw attention, though again this complication has not occurred in the present group.

SUMMARY

A group of 40 chronic severely disturbed psychotic patients has been given reserpine treatment and followed up for two years.

Thirteen of the 40 (32·5 per cent.) have been discharged, 5 have resumed full social responsibility and may be tentatively labelled "cured"; 8 others are successfully readjusted at home in a position of lessened responsibility. The mean duration of hospital stay in these 13 patients was 6·3 years and the mean length of the present remission is 9·3 months. So far there have been no readmissions in this group.

One patient died by suicide after discharge.

Short of actual discharge, 6 patients (15 per cent.) have achieved maximal hospital status, representing a much fuller and more socially integrated life than they enjoyed two years ago.

Some relevant factors in determining outcome in addition to the drug itself are discussed.

Twenty patients (50 per cent.) remain by and large unaltered.

ACKNOWLEDGMENTS

I am greatly indebted to Dr. J. P. Child, Physician Superintendent of St. Nicholas Hospital, Gosforth, not only for his interest in the treatment, but for the freedom of therapeutic manoeuvre which was afforded.

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"STEMETIL" IN THE TREATMENT OF CHRONIC PSYCHOTICS

By

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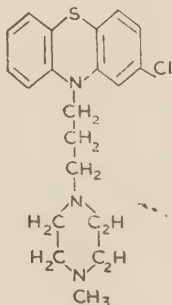
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THE range of Phenothiazine tranquillizers continues to expand. Among more recent introductions is prochlorperazine ("Stemetil") which incorporates a piperazine ring in the side chain.



Animal studies indicate no greater acute toxic effects, a more pronounced anti-emetic action, and less central depressant effect than chlorpromazine. Of considerable theoretical interest is the observation of a phenomenon resembling "flexibilitas cerea" in rats receiving doses of 10 mg./kg. subcutaneously.

A limited number of clinical trials have been carried out in the U.S.A. and on the continent. Vischer (1957), employing relatively small doses of prochlorperazine in general practice, claimed good results. Apart from a common "psychoneurotic overlay" the diversity of the clinical material constituting this series (38 patients) hardly justifies any conclusions beyond noting the absence of side-effects, apart from mild drowsiness, when the drug is given in doses of less than 40 mg. by mouth daily.

Wennersten (1956), reporting on the findings recorded by a large group of physicians in over 4,000 cases, was impressed by the results obtained. As in Vischer's series the clinical material seems to have covered an exceedingly wide field of organic and psychoneurotic disorder. Wennersten's own observations are confined to a study of 50 patients including a majority who had pain of one sort or another. This author concludes that prochlorperazine can control psychosomatic symptoms, reduce anxiety and tension, and diminish

awareness of pain associated with organic disease, and that it is usually effective in doses of 15-20 mg. daily.

Settel (1957) employing a similar range of dosage claims good results in senile patients with agitation and associated organic disease.

McAfoos' (1957) series consisted mainly of anxiety reactions in patients who could benefit from simple supportive measures without hospitalization. The daily dose of prochlorperazine was again between 15 mg. and 20 mg. The author concluded that the drug was equally effective in managing mild to moderately disturbed neurotic patients, as chlorpromazine or meprobamate, but, in common with the latter preparations, is not a substitute for psychotherapy.

It is not our intention to comment on the results claimed for prochlorperazine in these particular fields in which the complexities and pitfalls are only too obvious. Our interest lies rather in the use of the drug in the treatment of hospitalized psychotics.

Broussolle and Dubor (1956) compare its effects with those of chlorpromazine and formed the opinion that prochlorperazine was active in smaller doses in cases of paranoid and catonic schizophrenia, and in manic episodes. The dosage seldom exceeded 100 mg. daily, but above this level Parkinsonian-like effects, athetosis, and other transient neurological symptoms were observed. Among the latter special reference is made to attacks of "pseudo-tetany". This phenomenon has also been described in detail by Broussolle *et al.* (1957), occurring most often on the third day of treatment with a dose of 100 mg. daily. These authors give a comprehensive report on a series of over 200 mental hospital patients, mostly psychotics. They consider that prochlorperazine is more potent in certain cases than any of the other phenothiazine derivatives so far used. Its action in states of excitement, particularly in acute mania, is regarded as being unequalled. It is also claimed to be more effective than chlorpromazine in some chronic schizophrenic illnesses, and at least as active in others. The dosage employed varied between 75 and 200 mg. daily, and within this range it was found that the drug often gave rise to side-effects and was, therefore, recommended to be used only in hospital under strict medical supervision.

Borel *et al.* (1957) treated patients with depressive symptoms and obtained better results than might have been anticipated with chlorpromazine.

Lecomte *et al.* (1957) used prochlorperazine in cases of psychotic excitement and agitation, and again results seemed to have compared favourably with those obtained with chlorpromazine, and on a dosage of one-third to one-quarter of that of the latter drug.

The claims made for prochlorperazine as a more effective agent than chlorpromazine cannot be ignored and require further investigation. Of all the tranquillizers so far developed, chlorpromazine is unquestionably the most widely used in the treatment of chronic psychoses. Results of chlorpromazine therapy, therefore, provide a convenient yardstick in the assessment of other related compounds. Accordingly we decided to confine our clinical trial of prochlorperazine to patients who were receiving "maintenance" chlorpromazine. As previous experimental and clinical studies indicated that the newer compound might be about four times as potent as chlorpromazine, it was agreed to substitute in each case 25 mg. of prochlorperazine for 100 mg. of chlorpromazine. A reasonable measure of control was obtained by using special sugar-coated tablets identical in appearance to the standard 100 mg. tablets of chlorpromazine.

CASE MATERIAL

The first series to be treated with prochlorperazine consisted of 28 chronic male schizophrenics. The duration of illness was assessed at a mean of 12.5 years and the average age of the patients was 38.6 years. All had been receiving "maintenance" chlorpromazine for some months at least, at a minimum dosage of 300 mg. daily. Fifteen of these patients had been given chlorpromazine because they had been particularly turbulent, and they included two failed leucotomies (repeated in each case), and one patient who had not responded to insulin coma treatment. The remainder were more deteriorated, unoccupied, and with less aggressive tendencies, and included two failed leucotomy and two failed insulin cases. Reassessments were carried out after a six weeks' course of prochlorperazine and finally after a further six weeks on the original "maintenance" chlorpromazine administration.

Subsequently 24 female patients were treated and assessed in a similar manner. Again they were all chronic schizophrenics who had been ill for many years (mean 14.2) and whose average age was 48.1 years.

RESULTS

In general prochlorperazine appeared to maintain the improvement originally attributed to chlorpromazine, substantiating the view that "weight for weight" it is four times as potent as the latter drug. A more detailed analysis of symptom-traits did not reveal any significant changes, and apart from one exception all the cases in this series remained on their original gradings on a ten-point social rating scale. One female patient improved considerably and was able to leave hospital three weeks after the completion of her prochlorperazine course. She was then on her original dose of chlorpromazine which she has continued to take at home. Despite some misgivings about her condition prior to discharge from hospital, her remission has been maintained. During the period of the prochlorperazine trial it was noted that two female patients required additional night sedation, but otherwise there was no evidence of mental deterioration. One male patient who had been previously selected for leucotomy on account of progressive mental disturbance, was not relieved by prochlorperazine, and the operation was performed as planned.

SIDE-EFFECTS

One male patient (aged 44) developed a mild Parkinsonian-type reaction, chiefly affecting his gait. This complication gradually disappeared after the end of the trial period when chlorpromazine was substituted for prochlorperazine. Three female patients (aged 32, 46 and 53), also developed mild Parkinsonism while on prochlorperazine. These effects cleared up when the dose was reduced from 75 mg. to 50 mg. daily in two cases, and when prochlorperazine was omitted in favour of chlorpromazine in the other case. No "pseudo-tetany", as reported in the literature, was observed during the course of the trial.

SUMMARY

The effects of prochlorperazine ("Stemetil") in a series of 52 long-term hospital patients are compared with the results of chlorpromazine therapy. While the efficacy of prochlorperazine was confirmed, the trial did not reveal any precise indications for its use as compared with other phenothiazine tranquillizers.

We wish to thank all the members of the nursing staff at Knowle Hospital who helped us with carrying out the trial and Messrs. May & Baker Ltd. who supplied prochlorperazine tablets to our requirements.

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CLINICAL TRIAL OF PROMAZINE HYDROCHLORIDE AND ACETYLPROMAZINE IN CHRONIC SCHIZOPHRENIC PATIENTS

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INTRODUCTION

THE last few years have produced an extensive literature on the chemotherapy of schizophrenia (Foote, 1955; Shepherd and Watt, 1956; Wing, 1956; Kinross-Wright, 1957). Enthusiastic reports continue to appear in the American journals (Feldman, 1957; Winkelman, 1957) whereas a more cautious note is usually struck in psychiatric papers published in this country. The value of reserpine is now generally accepted (Moore and Mankin, 1957; Elkes, 1957) but it is not without dangerous side-effects (Wardell, 1957). Chlorpromazine is now used extensively and is probably a safer drug; the danger of jaundice has not proved to be a real contra-indication to the use of the drug. However, reports of deaths during treatment with chlorpromazine have been published (Stenback, 1956).

We therefore decided to run a trial of the later phenothiazine derivatives, promazine hydrochloride and acetylpromazine, since these are claimed to be less toxic. The value of drug trials in psychiatry is itself a matter of debate. There is no doubt that they are unscientific in that there are always uncontrolled (and in part unknown) variables. The benefit of the trial *per se* on staff and patients has been stressed (Houston, 1956; Forrest, 1957). The patients involved in the trial reported here are very "sophisticated", that is, the majority have been in previous trials of reserpine and azocyclonal. Again, the majority of these patients have had previous courses of E.C.T. or insulin coma therapy. Most of them are, in addition, involved in occupational or work therapy. Therefore, we anticipated no dramatic results in these patients, but designed the trial to answer two questions:

1. Is there any reason to change these patients from their present drugs to the newer phenothiazine derivatives?
2. Would weekly psychotherapeutic interviews help them more than a change of drug?

MATERIALS AND METHODS

Forty male patients suffering from schizophrenia were the subject of this trial. One patient had to be withdrawn after seven weeks because of an inter-current infection. The results in this case have not been included in the tables.

The mean age of the patients was 39 years 3 months with a range of 18 to 53 years. The mean length of stay in hospital was 13 years 4 months with a range of 3 to 24 years. The diagnosis was carefully reviewed in each case and confirmed. The trial was designed to extend over twelve weeks. During the first week each patient received lactose tablets i t.d.s. Eleven patients were now allocated (by use of random numbers) to the group (Group 1) who received lactose tablets i t.d.s., and weekly psychotherapeutic interviews. The other twenty-eight patients (Group 2) received acetylpromazine (75 mg. t.d.s.), promazine hydrochloride (100 mg. t.d.s.) and lactose tablets i t.d.s., each for a period of two weeks, the drug order being randomized. During the eighth week each patient received either amytal (gr. $\frac{3}{4}$ t.d.s.) or amphetamine (5 mg. t.d.s.). During the last four weeks each patient had equal periods on reserpine (1 mg. t.d.s.) and chlorpromazine (100 mg. t.d.s.). These allocations were also made by random selection. The results were scored on a Behaviour Chart (devised by Dr. E. A. M. Wood of this hospital). This chart takes note of the level of general activity, social activity, aggressive behaviour and habits (i.e. eating, dressing, toilet, etc.). The scoring is arranged so that good behaviour registers a minimum score and vice-versa. The scoring was done each morning and evening by the night and day nurse in charge of the ward. These nurses had all a personal knowledge of the patients concerned in the trial, in some cases going back over many years. We made no attempt to use dummy tablets. We explained the nature

TABLE I

Group 2: 28 Patients
Behaviour Ratings (Weekly Mean Scores) During Drug Periods
(Each 2 weeks except 4th period of 7 days)

Patient	Lactose	Acetyl Promazine	Promazine Hydro- chloride	Amph- etamine/ Amytal	Reserpine	Chlor- promazine	
1	..	57	57	55.5	60(A)	51.5	44.5
2	..	17.5	17.5	14.5	14(A)	14	14
4	..	55	46	52	56(B)	58.5	62
5	..	37	41	36.5	35(B)	35	35
7	..	24.5	23	21	20(A)	21.5	23
8	..	24	24.5	21.5	21(B)	24	21
9	..	7	7.5	7	7(B)	7	7
10	..	19	17.5	23	25(B)	21	21.5
12	..	21.5	21	21.5	22(A)	23.5	22
15	..	33	28	35.5	24(B)	17.5	19
16	..	68	65.5	66	64(A)	50	29.5
17	..	40	36.5	39.5	37(B)	35.5	35.5
18	..	20	21.5	21.5	21(B)	25	26.5
19	..	31	38	34.5	30(A)	32	27
20	..	21	19	19.5	15(B)	20	20
21	..	27.5	22	28.5	26(B)	28.5	23.5
22	..	14	14	15	16(A)	10.5	15
26	..	5.5	5	12	7(A)	7	7
27	..	7	7	7	7(A)	7	7
29	..	14	4.5	13.5	24(B)	26.5	28.5
30	..	28	26	28	22(B)	21	21.5
32	..	14	14	14	12(A)	11	14
33	..	0	0	0	0(A)	0	0
34	..	38	36.5	34	37(A)	30	7
36	..	35	35	31.5	7(A)	13	21
37	..	13	11.5	10	10(B)	49	34.5
38	..	0	0	0	0(B)	0	0
39	..	21.5	26.5	30	22(A)	21	19.5

A=Amytal.

B=Amphetamine.

of the trial to each of the nurses concerned, telling them that we wished to compare the effects of various drugs and that they would be able to recognize some of the tablets but not all.

Once the trial was complete we reviewed the Behaviour Charts and expressed the results in average weekly scores for each drug period.

RESULTS

These are shown in Table I (Group 2) which shows the average weekly scores for each patient. Recognizing that this method of scoring is inaccurate we decided that only changes which amounted to 25 per cent. of the resting or lactose scores would be considered significant. It will be seen that promazine hydrochloride and acetylpromazine were not effective in producing behaviour changes, except in Case 29 in whom acetylpromazine did produce a satisfactory improvement. By contrast reserpine was effective in eight patients and chlorpromazine in five patients. Amphetamine (4 patients) and amytal (1 patient) were also effective.

Of the eleven patients (Group 1) who had lactose for the seven weeks but in addition weekly interviews, five showed significant improvement (i.e. score improvement of 25 per cent. or more) as shown in Table II.

TABLE II

Group 1: 11 Patients

Behaviour Ratings (Weekly Mean Scores) for Patients Receiving Lactose Tablets and Weekly Interviews

Patient	Lac-	Lactose and Interview							Amphe-	Reserpine	Chlor-		
	tose								tamine/ .Amytal		promazine		
	Week	1	2	3	4	5	6	7	8		9	10	11
3	..	65	46	36	39	39	42	45	35(B)	49	47	35	57
6	..	41	40	21	23	18	28	30	28(B)	16	14	21	18
11	..	8	2	15	17	8	7	8	16(B)	15	14	15	14
13	..	21	27	33	35	35	35	35	35(B)	35	35	35	35
14	..	22	15	23	21	22	23	31	29(A)	29	25	21	26
23	..	32	36	21	20	21	28	22	24(B)	33	36	22	35
24	..	42	42	42	42	42	42	42	43(B)	28	28	42	31
25	..	27	27	14	14	14	17	17	(16A)	16	29	14	20
28	..	37	35	28	32	25	7	7	12(B)	7	5	18	13
31	..	7	7	7	7	7	7	7	7(B)	7	7	7	7
35	..	0	0	0	0	0	0	0	0(A)	0	0	0	0

A=Amytal.

B=Amphetamine.

Table III shows the results expressed for each drug and for the interview group as total numbers and percentage improvement.

TABLE III

Changes in Behaviour in Group 1 and Group 2

Drug		Group	Not Improved	Improved	Per cent. Improved	Total
Acetyl promazine	..	2	27	1	3.7	28
Promazine	..		28	0	0	28
Lactose and interviews		1	6	5	45.45	11

DISCUSSION

As noted in the introduction, we did not expect to get any very impressive results with the patients selected for this trial. However, the results do indicate that promazine hydrochloride and acetylpromazine are not going to replace reserpine and chlorpromazine in the treatment of chronic schizophrenia. Thus the answer to our first question is "No".

The apparent benefit derived from weekly interviews, though based on a small number of patients, seems to be the most significant observation of this trial. From the historical standpoint we are in process of rediscovering what was known 150 years ago, namely that chronic patients are responsive to friendly interest and encouragement. But to be effective such interest must be directed within a conceptual framework regarding the nature of schizophrenia itself. If we believe in the psychogenesis of schizophrenia then clearly a psychotherapeutic approach is appropriate, though not necessarily effective. Such work has been reported by Bateson and his co-workers at Palo Alto (1956). If, on the other hand, we suspect that this disease results from agentically determined enzymatic disturbance as suggested by Fabing (1958) then research must be directed to the underlying defect, whether it relates to adrenal dysfunction or indole metabolism. But for the problem of the chronic schizophrenic patients who are in our wards now research into the possible biochemical basis of the disease is likely to be irrelevant. These patients represent the late stages of complex biological processes and without doubt much of the behaviour we regard as typical of these chronic patients is due to hospitalization. The remarkable benefits resulting from physical methods of treatment in the last thirty years have tended to place the emphasis on treatment of new admissions and relegate the chronics to the level of custodial care. The chronic wards are no longer places of violence, destruction and degraded personal habits, but our impression is that, though quiet and orderly, the chronic wards have not yielded the quota of recoveries that might have been anticipated. Possibly this is because neuroleptic drugs do not *per se* affect motivation; nor do they supply what the chronic schizophrenic patient needs and what his environment often denies, the opportunity for social intercourse and the establishment of interpersonal relationships. Part of this defect may be supplied by work therapy (Bickford, 1955; Clark, 1956) and by creating a therapeutic community as was done by T. P. Rees at Warlingham Park. That psychotherapy itself may be effective whether in the form of group discussion or in the form of participation in the work of an occupational centre, is suggested by the work of Freeman and his co-workers (1958). Undoubtedly group therapy for schizophrenics in one form or another is now being practised in many mental hospitals. One of us has had the opportunity to run such a group, mostly chronic schizophrenics. Five patients previously involved in the trial here reported have since been treated in the group. In this setting they have made substantially more progress than they did in response to any of the drugs employed.

A large amount of money is spent each year now on the chemotherapy of chronic schizophrenia. Similarly a large amount of energy is devoted to the trial of new drugs on this population. We suggest that this time and money might be better employed in securing more extensive work, social and psychotherapy, for these unfortunate people.

SUMMARY

1. A clinical trial of acetylpromazine and promazine hydrochloride in 39 long-term male schizophrenics is described.

2. The trial was designed to compare the changes in behaviour induced by these drugs with those resulting from chlorpromazine and reserpine on the one hand and weekly psychotherapeutic interviews on the other.

3. The results suggest that acetylpromazine and promazine hydrochloride will not replace chlorpromazine and reserpine in the treatment of schizophrenia. Also that the effect of weekly interviews may be greater than that of changing about the patient's drug.

4. These results are discussed in the light of current views on work therapy and group activity.

ACKNOWLEDGMENTS

We would like to thank Dr. T. A. Munro, Physician Superintendent, for permission to publish, Dr. E. A. M. Wood for allowing us to use the Behaviour Chart, which he had designed, and the Nursing Staff of the Royal Edinburgh Hospital for their co-operation in running this trial.

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Reviews

The physical foundation of biology. By WALTER M. ELSASSER. Pergamon Press, London, 1958. Pp. 219. 30s.

This book is difficult to review, for it deserves praise and criticism in a somewhat complex mixture. Let me start by simply describing it.

The author is Professor of Theoretical Physics at the University of California, and he brings to his subject all the specialized knowledge that is to be expected. The book is not a textbook on physics applied to biology but rather the reactions of a physicist to certain outstanding phenomena in the biological world, especially to those of regulation and co-ordination as they occur in embryogenesis and in cerebral function, i.e. in normal adult behaviour. Elsasser thus attacks an important biological problem with the fundamental methods of physics.

"Physics" today, however, is somewhat different from the physics of even twenty years ago. Elsasser gives little attention to the older topics of biophysics (energy, work, mechanics); rather is he concerned with the modern cybernetic aspects of information-flow, regulation, integration—the more patterned, orderly, purposeful aspects of behaviour. To make clear how he applies these concepts to biological systems, he devotes about half of the book to a description of well-known facts about feed-back, information-stores, computing machines, and similar physical mechanisms, so as to show that *certain forms of behaviour can logically be related to certain forms of mechanism*.

To this extent the book contains little that is new. He has, however, also a particular thesis to propound. He has read Bergson's *La Matière et la Mémoire* (1896) and has evidently been much impressed by it. Now he propounds essentially the same thesis, with supporting evidence based on modern methods for measuring quantities of information. He considers the brain as a material system, examines its physico-chemical and neuronc nature, and estimates how much information such a material system would be able to store. He also considers the normally purposive and intelligent adult behaviour and estimates how much information is thereby required. He finds that the brain, as a material system, has a capacity for information that is significantly less than that required for the behaviour of the normal adult. He draws the deduction (p. 145): "... organisms . . . do not store information by mechanistic means." In other words (mine) he considers that the facts show that there is more in adult human behaviour than can be accounted for by the brain—a conclusion of great scientific interest if it can be well proven.

His demonstration, we should notice, is based on fundamentally scientific methods, even though it leads to a somewhat vitalistic conclusion; for all his facts are objectively demonstrable and capable of measurement. His method is basically similar to what one would use if it were suggested that the amount of energy a human being obtained from his food was not sufficient to account for the energy that he put out in his activities: one would measure the energy-value of the food taken in, also the amount of energy put out, and one would compare the two numbers. Similarly Elsasser estimates the amount of information holdable by the brain, and the amount necessary for normal behaviour, and he compares the two estimates. The method on which his thesis rests is thus essentially scientific. It leads to a deduction of considerable interest to those who are working on the correlation between brain and behaviour.

So far I have attempted simply to describe the book in non-committal terms. I now turn to the more difficult question of the book's value as a contribution to cybernetics and science. Here my opinion is somewhat mixed, for the book's quality varies widely, even from paragraph to paragraph. When the author is

dealing with his own professional topics he is, naturally, accurate and well informed. Much of the book is thus well worth reading as an explanation of the physicist's methods to biology. But when he comes to the biological aspect his grasp weakens, perhaps to the point of inadequacy. The inadequacy is most damaging, unfortunately, in the very core of his thesis, namely in his attempts to make the two estimates on which his thesis rests. Since, in my opinion, there is one fault that is outstanding, and as it occurs repeatedly in present-day cybernetic writings, I propose to examine the matter so as to show clearly what is wrong.

What sample space?

We are all agreed (Elsasser, the reviewer, and others) that by "information" we mean "as defined by Shannon". Now Shannon's definition makes *essential* use of a sample space, which is assumed given; that is, information (in any particular case) can be measured by a worker only *after* he has specified a definite set of events, each with an assigned probability. Change the sample space and the measure changes its numerical value. (Sometimes the worker may reasonably say that he is not interested in the particular values of the probabilities but only in the maximal value that the entropy can attain; even in this case he must still define the *number* of events in the sample space, for if this number changes so will the value of his estimate.) Unfortunately, this sample space is sometimes so obvious as to be hardly worth mentioning; then the worker gets into the habit of not noticing the sample space explicitly, and the risk of misunderstandings arises. The experience of the theoretical physicist is particularly unfortunate in this respect, for when he discusses the systems of statistical mechanics his sample space, on which his probabilities are based, is usually so simple and obvious that he gives it hardly a thought and often no mention. Thus he becomes accustomed to talk freely of probability and entropy without defining their sample spaces. In his context there is usually only one likely sample space anyway, but there are other systems, especially the biological, in which *a multiplicity of sample spaces is possible*. Then occur discussions of the entropy, or information content, of the protein molecule, or a synapse, or a neuron, in which the sample space, and thus the information, is neither obvious nor carefully defined; then confusion starts. Here the fundamental fact is that *specifying a physical object does not specify a unique sample space, and therefore not a unique value for the information content*.

The point can be made incisively with one simple question: how much information is there in a relay? Immediately we realize that there are at least two plausible answers, which differ grossly. (They are worth consideration in detail, for they illustrate the matter forcibly.) First we can regard the relay as an assembly of atoms (of which there are about 10^{24} , chiefly iron and copper). Each of these, with its electrons, may be in various states; if (to get a definite example) each can be in either of two states, independently of the others, then the variety possible is 10^{24} bits. On the other hand, another worker may prefer to regard it as something that has the two states "energized" and "not-energized", with variety of 1 bit. Thus (to write it out in full) failure to specify the sample space, even in this simple little example, leaves us unable to decide whether the information content of the relay should be counted as

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or as 1,000,000,000,000,000,000,000,000!

Such is the penalty for being vague about the sample space.

Similarly, a roulette wheel does not, as a physical object, indicate a unique sample space; in consequence, neither does it indicate a unique quantity of information.

In the cases just mentioned, the context will often hint at the appropriate sample space, and confusion may often be avoided. But when the physical object is biological—a virus, or a synapse, or a neuron, or a human being—the possible sample spaces become so many and varied that nothing less than explicit mention

will ensure freedom from uncertainty; failing it, the values of "information in the organism" have such a vast range as to be practically unrestricted. To demand that the sample space be specified is not to demand an extravagant and unnecessary accuracy: it is to demand simply that the worker say unambiguously which of the many possible "informations" he is talking about.

Unfortunately, as I said, the physicist's experience with physical systems (in thermodynamics and statistical mechanics) does not warn him of the grave consequences, in biological systems, of a failure to define sufficiently his sample space. Elsasser nowhere gives serious attention to these questions. As a result, much of the book's discussion of cybernetics, as applied to biological systems, is infected with a fundamental vagueness that is fatal to his argument. Though the book contains much that might be used as a popularly written introduction to information theory, one cannot recommend it to the serious beginner lest he should fall into the bad habit, only too common at the present time, of talking glibly about information without being able to say to what sample space he is referring. The book, in this, is perpetuating the crudities of ten years ago instead of achieving the rigour and accuracy demanded by the standards of today.

My opinion on the book's main thesis now follows inevitably. If a worker wants to revolutionize modern biology and neurophysiology on the grounds that measurements applied to the living organism give results incompatible with accepted theory, then clearly the onus is on him to make the measurements with sufficient accuracy to demonstrate that the discrepancy is significant. Has Elsasser done this? The example of the relay shows the order of magnitude of the error that can occur if the sample space is not adequately specified. Since he nowhere specifies the sample spaces of his estimates of the information content, nor even suggests about which end of the scale he is thinking, his thesis must, in my opinion, be regarded as not proven.

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Communication, organization, and science. By JEROME ROTHSTEIN; with a Foreword by C. A. Muses. The Falcon's Wing Press, Colorado, 1958. Pp. xcvi+110. \$3.50.

This book, though on a subject of fundamental importance in modern science, cannot be recommended. Its Foreword, of 96 pages, scatters sparkling ideas like a firework but leaves (at least in the reviewer's mind) no enduring trace. The other half of the book juggles speculatively, and on a purely verbal level, with various topics in communication and organization, but does so with a looseness and lack of rigour that must be outspokenly condemned as being inadmissible today. The pioneer work of Shannon and Wiener has given us a foundation for a science of organization that is absolutely rigorous and secure for those who will *use* the rigour. But adherence to the rigorous form demands work and trouble—how much easier it is, both for author and reader, if the discussion is merely a rambling chat about communication and organization, with no trouble being taken to see whether what is said on one page matches exactly what is said on another!

The beginner should certainly not read this book, for it will lead him into a slovenly looseness of thought from which he may never recover. The advanced worker need not read it, for he will probably have encountered most of the ideas elsewhere.

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